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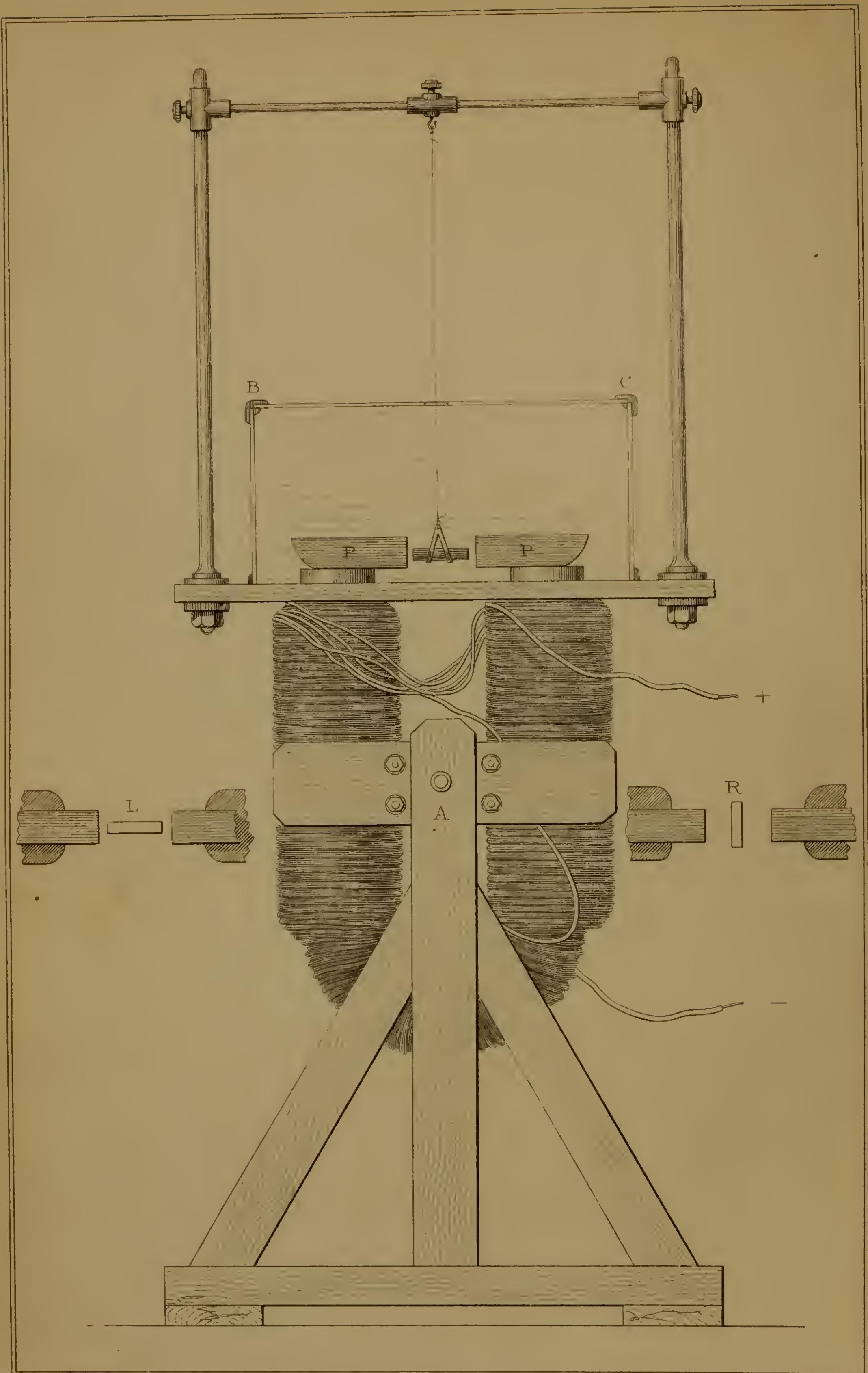

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THE ROYAL INSTITUTION MAGNET.

London. Longmans & Co.

RESEARCHES

ON

DIAMAGNETISM

AND

MAGNE-CRYSTALLIC ACTION,

INCLUDING THE QUESTION OF

DIAMAGNETIC POLARITY.

BY

JOHN TYNDALL, LL.D. F.R.S.

PROFESSOR OF NATURAL PHILOSOPHY IN
THE ROYAL INSTITUTION.

LONDON:
LONGMANS, GREEN, AND CO.
1870.

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LETTERS, ESSAYS, AND REVIEWS RELATING TO MAGNETISM AND ELECTRICITY.

LIST OF PLATES.

Errata.

Page 135, line 23, *for* Plate IV. *read* Plate II*a*.

Page 298, *for* Plate III. *read* Plate V*a*.

NOTE.

The Electro-magnet represented in the Frontispiece is that generally used by Faraday in his researches on Diamagnetism. He, however, employed a retort stand for suspension, and he covered the poles by a square glass shade, BC, to protect the suspended body from currents of air.

The magnet is part of a link of a great chain-cable; its section is a distorted square, rounded off at the corners. The magnet, coil inclusive, weighs 272 pounds.

On the ends of the magnet stand two pieces of iron, PP, which are the movable poles. They represent those most commonly used by Faraday. Various other poles, however, with rounded, conical, and chisel ends, and some with perforations to allow a beam of light to pass through them, and across the magnetic field, were employed from time to time.

Right and left of the drawing, at R and L, are shown in plan the pole ends, with a little bar in its two characteristic positions, axial and equatorial, between them. The magnet is at present supported by the pivot A, which permits its arms to be turned into the horizontal position; this arrangement has been found useful in experiments on Magneto-electric Induction.

J. T.

Royal Institution, *March* 1870.

INTRODUCTION.

In the Preface to the third edition of my work on Heat occur the following words:—'Within the coming year (1869) I hope to collect and publish the original memoirs on Experimental Physics, which I have communicated to the "Philosophical Transactions" and the "Philosophical Magazine" during the last eighteen years.' The fulfilment of this hope has been retarded by an accident which prevented me from working during the whole of last autumn; nor am I now able to offer more than an instalment of the work proposed. The present memoirs, however, do not suffer by their severance from those on heat and other subjects: they are tolerably complete in themselves.

An extract from another little book of mine, entitled 'Faraday as a Discoverer,'* will form as suitable an introduction to this one as I could at present write.

On December 18, 1845, Faraday communicated to the Royal Society a memoir on the 'Magnetic Condition of all Matter.' 'Before the pole of an electro-magnet, he suspended a fragment of his famous heavy glass; and observed that when the magnet was powerfully excited the glass fairly retreated from the pole. It was a clear case of magnetic *repulsion*. He then suspended a bar of the glass between two poles; the bar retreated when the poles were excited, and set its length *equatorially* or at

* New Edition, pp. 109–132.

right angles to the line joining them. When an ordinary magnetic body was similarly suspended, it always set *axially*, that is, from pole to pole.

'He called those bodies which were repelled by the poles of a magnet, *diamagnetic* bodies; using this term in a sense different from that in which he applied it in his memoir on the magnetisation of light. The term *magnetic* he reserved for bodies which exhibited the ordinary attraction. He afterwards employed the term magnetic to cover the whole phenomena of attraction and repulsion, and used the word *paramagnetic* to designate magnetic action like that of iron.

'Isolated observations by Brugmanns, Becquerel, le Baillif, Saigy, and Seebeck had indicated the existence of a repulsive force exercised by the magnet on two or three substances; but these observations, which were unknown to Faraday, had been permitted to remain without extension or examination. Having laid hold of the fact of repulsion, he immediately expanded and multiplied it. He subjected bodies of the most various qualities to the action of his magnet:—mineral salts, acids, alkalis, ethers, alcohols, aqueous solutions, glass, phosphorus, resins, oils, essences, vegetable and animal tissues, and found them all amenable to magnetic influence. No known solid or liquid proved insensible to the magnetic power when developed in sufficient strength. All the tissues of the human body, the blood—though it contains iron—included, were proved to be diamagnetic. So that if you could suspend a man between the poles of a magnet, his extremities would retreat from the poles until his length became equatorial.

'Faraday's thoughts ran intuitively into experimental combinations, so that subjects whose capacity for experimental treatment would to most minds seem to be exhaustible in a moment, were shown by him to be all but inexhaustible. He had an object in view, the first step towards which was the proof that the principle of Archimedes is true of magnetism. He formed magnetic solutions of various degrees of strength, placed them between the poles of his magnet, and suspended in the solutions various magnetic bodies. He proved that when the solution is stronger than the body plunged in it, the body, though magnetic, is repelled; and when an elongated piece of it is surrounded by the solution it sets, like a diamagnetic body,

equatorially between the excited poles. The same body when suspended in a solution of weaker magnetic power than itself is attracted as a whole, while an elongated portion of it sets axially.

'And now theoretic questions rush in upon him. Is this new force a true repulsion, or is it merely a differential attraction? Might not the apparent repulsion of diamagnetic bodies be really due to the greater attraction of the medium by which they are surrounded? He tries the rarefaction of air, but finds the effect insensible. He is averse to ascribing a capacity of attraction to space, or to any hypothetical medium supposed to fill space. He therefore inclines, but still with caution, to the opinion that the action of a magnet upon bismuth is a true and absolute repulsion, and not merely the result of differential attraction. And then he clearly states a theoretic view sufficient to account for the phenomena. "Theoretically," he says, "an explanation of the movements of the diamagnetic bodies, and all the dynamic phenomena consequent upon the action of magnets upon them, might be offered in the supposition that magnetic induction caused in them a contrary state to that which it produced in magnetic matter." That is to say, while in ordinary magnetic influence the exciting pole excites adjacent to itself the contrary magnetism, in diamagnetic bodies the adjacent magnetism is the same as that of the exciting pole. This theory of reversed polarity, however, does not appear to have ever laid deep hold of Faraday's mind; and his own experiments failed to give any evidence of its truth. He therefore subsequently abandoned it, and maintained the *non-polarity* of the diamagnetic force.

'He then entered a new, though related field of inquiry. Having dealt with the metals and their compounds, and having classified all of them that came within the range of his observation under the two heads magnetic and diamagnetic, he began the investigation of the phenomena presented by crystals when subjected to magnetic power. The action of crystals had been in part theoretically predicted by Poisson,* and actually discovered by Plücker, whose beautiful results, at the period which we have now reached, profoundly interested all scientific men. Faraday had been frequently puzzled by the deportment of bismuth, a highly crystalline metal. Sometimes elongated masses

* See Sir William Thomson, 'Phil. Mag.' 1851, and page 66 of this volume.

of the substance refused to set equatorially, sometimes they set persistently oblique, and sometimes even, like a magnetic body, from pole to pole. "The effect," he says, "occurs at a single pole; and it is then striking to observe a long piece of a substance so diamagnetic as bismuth repelled, and yet at the same moment set round with force, axially, or end on, as a piece of magnetic substance would do." The effect perplexed him; and in his efforts to release himself from this perplexity, no feature of this new manifestation of force escaped his attention. His experiments are described in a memoir communicated to the Royal Society on the 7th of December, 1848.

'I have worked long myself at magne-crystallic action, amid all the light of Faraday's and Plücker's researches. The papers now before me were objects of daily and nightly study with me eighteen or nineteen years ago; but even now, though their perusal is but the last of a series of repetitions, they astonish me. Every circumstance connected with the subject; every shade of deportment; every variation in the energy of the action; almost every application which could possibly be made of magnetism to bring out in detail the character of this new force, is minutely described. The field is swept clean and hardly anything experimental is left for the gleaner. The phenomena, he concludes, are altogether different from those of magnetism or diamagnetism; they would appear, in fact, to present to us "a new force, or a new form of force, in the molecules of matter," which for convenience sake he designates by a new word, as "the *magne-crystallic* force."

'He looks at the crystal acted upon by the magnet. From its mass he passes, in idea, to its atoms, and he asks himself whether the power which can thus seize upon the crystalline molecules, after they have been fixed in their proper positions by crystallising force, may not, when they are free, be able to determine their arrangement? He therefore liberates the atoms by fusing the bismuth. He places the fused substance between the poles of an electro-magnet, powerfully excited; but he fails to detect any action. I think it cannot be doubted that an action is exerted here,* that a true cause comes into play; but its magnitude is not such as sensibly to interfere with the force

* Plücker thought he had established an action. I failed to obtain it.

of crystallisation. "Perhaps," adds Faraday, "if a longer time were allowed and a permanent magnet used, a better result might be obtained. I had built many hopes upon the process." This expression, and his writings abound in such, illustrates what has been already said regarding his experiments being suggested and guided by his theoretic conceptions. His mind was full of hopes and hypotheses, but he always brought them to an experimental test. The record of his planned and executed experiments would show a high ratio of hopes disappointed to hopes fulfilled; but every case of fulfilment abolished all memory of defeat; disappointment was swallowed up in victory.

'After the description of the general character of this new force, Faraday states with the emphasis here reproduced its mode of action: "The *law* of action appears to be that *the line or axis of* MAGNE-CRYSTALLIC *force* (being the resultant of the action of all the molecules) *tends to place itself parallel, or as a tangent, to the magnetic curve, or line of magnetic force, passing through the place where the crystal is situated.*" The magne-crystallic force, moreover, appears to him "to be clearly distinguished from the magnetic or diamagnetic forces, in that it causes neither approach nor recession, consisting not in attraction or repulsion, but in giving a certain determinate position to the mass under its influence." And then he goes on "very carefully to examine and prove the conclusion that there was no connection of the force with attractive or repulsive influences." With the most refined ingenuity he shows that, under certain circumstances the magne-crystallic force can cause the centre of gravity of a highly magnetic body to retreat from the poles, and the centre of gravity of a highly diamagnetic body to approach them. His experiments root his mind more and more firmly in the conclusion that it is "neither attraction nor repulsion causes the set or governs the final position" of the crystal in the magnetic field. That the force which does so is therefore "distinct in its character and effects from the magnetic and diamagnetic forms of force. On the other hand," he continues, "it has a most manifest relation to the crystalline structure of bismuth and other bodies, and therefore to the power by which their molecules are able to build up the crystalline masses."

'And here follows one of those expressions which characterise the conceptions of Faraday in regard to force generally:—"It

appears to me impossible to conceive of the results in any other way than by a mutual reaction of the magnetic force, and the force of the particles of the crystal upon each other." He proves that the action of the force though thus molecular is an action at a distance; he shows that a bismuth crystal can cause a freely suspended magnetic needle to set parallel to its magne-crystallic axis. "But though it thus takes up the character of a force acting at a distance, still it is due to that power of the particles which makes them cohere in regular order and gives the mass its crystalline aggregation, which we call at other times the attraction of aggregation, and so often speak of as acting at *insensible* distances." Thus he broods over this new force, and looks at it from all possible points of inspection. Experiment follows experiment, as thought follows thought. He will not relinquish the subject as long as a hope exists of throwing more light upon it. He knows full well the anomalous nature of the conclusion to which his experiments lead him. But experiment to him is final, and he will not shrink from the result. "This force," he says, "appears to me to be very strange and striking in its character. It is not polar, for there is no attraction or repulsion." And then as if startled by his own utterance, he adds:—"What is the nature of the mechanical force which turns the crystal round and makes it affect a magnet?" . . . "I do not remember," he continues, "heretofore such a case of force as the present one where a body is brought into position only, without attraction or repulsion."

'Plücker, the celebrated geometer already mentioned, who pursued experimental physics for many years of his life with singular devotion and success, visited Faraday in those days, and repeated before him his beautiful experiments on magneto-optic action. Faraday repeated and verified Plücker's observations, and concluded, what he at first seemed to doubt, that Plücker's results and magne-crystallic action have the same origin.

'Plücker's and Faraday's investigations filled all minds at the time here referred to, and towards the end of 1849, Professor Knoblauch and myself commenced a joint investigation of the entire question. Long discipline was necessary to give us due mastery over it. Employing a method proposed by Dove, we examined the optical properties of our crystals ourselves; and

the optical observations went hand in hand with our magnetic experiments. The number of these experiments was very great, but for a considerable time no fact of importance was added to those already published. At length, however, it was our fortune to meet with various crystals whose deportment could not be brought under the laws of magne-crystallic action enunciated by Plücker. We also discovered instances which led us to suppose that the magne-crystallic force was by no means independent, as alleged, of the magnetism or diamagnetism of the mass of the crystal. Indeed the more we worked at the subject the more clearly did it appear to us that the deportment of crystals in the magnetic field was due, not to a force previously unknown, but to the modification of the known forces of magnetism and diamagnetism by crystalline aggregation.

'An eminent example of magne-crystallic action adduced by Plücker and experimented on by Faraday, was Iceland spar. It is what in optics is called a *negative* crystal, and according to the law of Plücker, the axis of such a crystal was always repelled by a magnet. But we showed that it was only necessary to substitute, in whole or in part, carbonate of iron for carbonate of lime, thus changing the magnetic, but not the optical character of the crystal, to cause the axis to be attracted. That the deportment of magnetic crystals is exactly antithetical to that of diamagnetic crystals isomorphous with the magnetic ones, was proved to be a general law of action. In all cases, the line which in a diamagnetic crystal set equatorially, always set itself in an isomorphous magnetic crystal axially. By mechanical compression other bodies were also made to imitate the Iceland spar.

'These and numerous other results bearing upon the question were published at the time in the "Philosophical Magazine" and in "Poggendorff's Annalen;" and the investigation of diamagnetism and magne-crystallic action was subsequently continued by me in the laboratory of Professor Magnus of Berlin. But it required long subsequent effort to subdue the complications of magne-crystallic action, and to bring under the dominion of elementary principles the vast mass of facts which the experiments of Faraday and Plücker had brought to light. It was proved by Reich, Edmond Becquerel, and myself, that the condition of diamagnetic bodies in virtue of which they were

repelled by the poles of a magnet, was excited in them by those poles; that the strength of this condition rose and fell with, and was proportional to, the strength of the acting magnet. It was not then any property possessed permanently by the bismuth, and which merely required the development of magnetism to act upon it, that caused the repulsion; for then the repulsion would have been simply proportional to the strength of the influencing magnet, whereas experiment proved it to augment as the square of the strength. The capacity to be repelled was therefore not inherent in the bismuth, but *induced*. Thus far an identity of action was established between magnetic and diamagnetic bodies. After this the deportment of magnetic bodies, "normal" and "abnormal," crystalline, amorphous, and compressed, was compared with that of crystalline, amorphous, and compressed diamagnetic bodies; and by a series of experiments, executed in the laboratory of this Institution, the most complete antithesis was established between magnetism and diamagnetism. This antithesis embraced the quality of polarity,—the theory of reversed polarity, first propounded by Faraday, being proved to be true. The discussion of the question was very brisk. On the continent Professor Wilhelm Weber was the ablest and most successful supporter of the doctrine of diamagnetic polarity; and it was with an apparatus, devised by him, at my request, and constructed for me under his own superintendence, by Leyser of Leipzig, that the last demands of the opponents of diamagnetic polarity were satisfied. The establishment of this point was absolutely necessary to the explanation of magne-crystallic action.

'With that admirable instinct which always guided him, Faraday had seen that it was possible, if not probable, that the diamagnetic force acts with different degrees of intensity in different directions, through the mass of a crystal. In his studies on electricity he had sought an experimental reply to the question whether crystalline bodies had not different specific inductive capacities in different directions, but he failed to establish any difference of the kind. His first attempt to establish differences of diamagnetic action in different directions through bismuth, was also a failure: but he must have felt this to be a point of cardinal importance, for he returned to the subject in 1850, and proved that bismuth was repelled with

different degrees of force in different directions. It seemed as if the crystal were compounded of two diamagnetic bodies of different strengths, the substance being more strongly repelled across the magne-crystallic axis than along it. The same result was obtained independently, and extended to various other bodies, magnetic as well as diamagnetic, and also to compressed substances, a little subsequently by myself. The law of action in relation to this point is, that in diamagnetic crystals, the line along which the repulsion is a maximum sets equatorially in the magnetic field; while in magnetic crystals the line along which the attraction is a maximum sets from pole to pole. Faraday had said that the magne-crystallic force was neither attraction nor repulsion. Thus far he was right. It was neither, taken singly, *but it was both.* By the combination of the doctrine of diamagnetic polarity with these differential attractions and repulsions, and by paying due regard to the character of the magnetic field, every fact brought to light in the domain of magne-crystallic action received complete explanation. The most perplexing of those facts were shown to result from the action of mechanical couples, which the proved polarity both of magnetism and diamagnetism brought into play. Indeed the thoroughness with which the experiments of Faraday were thus explained, is the most striking possible demonstration of the marvellous precision with which they were executed.'

These researches were begun at Marburg at the close of 1849, the first paper of the series being a joint one with my friend Professor Knoblauch. The work was continued afterwards by myself in Berlin, and completed in England.

Some purely verbal alterations have been made, and here and there a superfluous passage has been omitted; but the memoirs remain, nearly in word and wholly in substance, what they were when they first appeared.

For the use of plates and blocks, my best thanks are due to the Council of the Royal Society and the proprietors of the 'Philosophical Magazine.'

FIRST MEMOIR.

THE MAGNETO-OPTIC PROPERTIES OF CRYSTALS AND THE RELATION OF MAGNETISM AND DIA-MAGNETISM TO MOLECULAR ARRANGEMENT.

By John Tyndall and Hermann Knoblauch.*

In the year 1846 our views of magnetic action received, through the researches of Faraday, an extraordinary expansion. The experiments of Brugmans, Le Baillif, Seebeck, and Becquerel had already proved the power to be active beyond the limits usually assigned to it; but these experiments were isolated and limited in number. Faraday was the first to establish the broad fact, that there is no known body indifferent to magnetic influence when the latter is strongly developed. The nature of magnetic action was then found to be twofold, attractive and repulsive; thus dividing bodies into two great classes, which are respectively denominated *magnetic* and *diamagnetic*.

The representative of the former class is *iron*, which, being brought before the single pole of a magnet, is attracted; the representative of the latter class is *bismuth*, which, being brought before the single pole of a magnet, is repelled.

If a little bar of iron be hung freely between the two poles of a magnet, it will set its longest dimension in the line joining the poles; a little bar of bismuth, on the contrary, will set its longest dimension at right angles to the line joining the poles. The position of the iron is termed by Mr. Faraday the *axial*; the position of the bismuth, the *equatorial*. We shall have occasion to use these terms.

These discoveries, opening, as they did, a new field in physical science, invited the labours of scientific men on the Continent. Weber, Œrsted, Reich, and others have occupied themselves with the subject. But, if we except the illustrious

* Phil. Mag., July 1850.

discoverer himself, there is no investigator in this branch of science whose labours have been so richly rewarded as those of Professor Plücker of Bonn.

In 1847 M. Plücker had a magnet constructed of the same size and power as that described by Mr. Faraday,* his object being to investigate the influence of the fibrous constitution of plants upon their magnetic deportment. While conducting these experiments, he was induced to try whether crystalline structure exercised an influence. 'The first experiment,' says M. Plücker, 'gave an immediate and decided reply.'

Following up his investigations with crystals, he was led to the affirmation of the following two laws :—

'*When any crystal whatever with one optic axis is brought between the poles of a magnet, the axis is repelled by each of the poles; and if the crystal possess two axes, each of these is repelled, with the same force, by the two poles.*

'*The force which causes this repulsion is independent of the magnetism or diamagnetism of the mass of the crystal; it decreases with the distance more slowly than the magnetic influence exerted by the poles.*'†

It is, perhaps, worth explaining that if, on exciting the magnet, the optic axis take up the *axial* position, it is said to be attracted; if the *equatorial*, it is said to be repelled.

The first experiment of M. Plücker, which led to the affirmation of these laws, was made with tourmaline. A plate of the crystal which had been prepared for the purposes of polarisation, twelve millimetres long, nine wide, and three thick, was suspended by a silk fibre between the poles of an electro-magnet. On sending a current round the latter, the plate, which was magnetic, set itself as an ordinary magnetic substance would do, with its longest dimension from pole to pole. The optic axis of the crystal, thus suspended, was vertical.

On hanging the crystal, however, with its optic axis horizontal, when the magnet was excited, the plate stood no longer as a magnetic substance, but as a diamagnetic; its longest dimension being at right angles to the line joining the poles. The optic axis of the crystal was found to coincide with its

* Phil. Mag., vol. xxviii. p. 396.

† Poggendorff's *Annalen*, vol. lxxii. p. 75.

length, and the peculiar deportment was considered as a proof that the optic axis was repelled.

This law was further established by experiments with Iceland spar, quartz, zircon, beryl, &c., and, as above stated, included crystals of all kinds, both optic positive and negative. It has, however, lately undergone considerable modification at the hands of M. Plücker himself. In a letter to Mr. Faraday, which appears in page 450, vol. xxxiv. of the 'Philosophical Magazine,' he expresses himself as follows :—

'The first and general law I deduced from my last experiments is the following one :—"There will be *either repulsion or attraction* of the optic axes by the poles of a magnet, according to the crystalline structure of the crystal. If the crystal is a *negative* one, there will be *repulsion*; if it is a *positive* one, there will be *attraction*."'*

This law applies to crystals possessing two optic axes, each of the said axes being attracted or repelled according as the crystal is positive or negative. It will simplify the subject if we regard the line bisecting the acute angle enclosed by the two axes as the resultant of attraction or repulsion; for the sake of convenience, we shall call this the *middle line*. In positive crystals, therefore, the middle line, according to the above law, must stand *axial*; in negative crystals, *equatorial*. It is also evident that the plane passing through the optic axes must, in the one class of crystals, stand from pole to pole, in the other class at right angles to the line joining the poles.

In explaining this new modification of the law, M. Plücker lays particular emphasis upon the fact that the attraction or repulsion is the result of an independent force, connected in no way with the magnetism or diamagnetism of the mass of the crystal; and this view is shared by Mr. Faraday, who, in expressing his concurrence with M. Plücker, denominates the force in question an 'optic axis force.'†

The experiments described in our first paper upon this subject‡ furnish, we conceive, sufficient ground of dissent from these views. In the case of five crystals of pure carbonate of lime (Iceland spar), we found the law of Plücker strictly verified, all five crystals being diamagnetic; on replacing,

* Phil. Mag., vol. xxxiv. p. 450.

† Phil. Trans. 1849, p. 32.

‡ Phil. Mag., vol. xxxvi. p. 178. A short preliminary notice printed further on.]

however, a portion of the carbonate of lime by carbonate of iron, nature herself being the chemist in this case, the crystal was no longer diamagnetic, but magnetic; in every other respect it was physically unchanged; its optical properties remained precisely as before, the crystal of carbonate of lime and the crystal of carbonate of lime and iron being both negative. In the one case, however, the optic axis was attracted; in the other the said axis was repelled, the attraction being evidently caused by the passage of the crystal from the diamagnetic into the magnetic state.

We have examined other crystals of the same form as Iceland spar, both magnetic and diamagnetic. In all cases the former act in a manner precisely similar to the carbonate of lime and iron already described, while the latter behave as the pure carbonate of lime. The following are examples.

Nitrate of Soda.—This crystal is of the same form as carbonate of lime, and, like it, diamagnetic. Its deportment is in every respect the same. A rhombus cloven from the crystal and suspended horizontally between the poles sets its longer diagonal axial. Suspending the full crystal between the poles, with its optic axis horizontal, on exciting the magnet this axis sets itself equatorial.

Breunnerite.—This is a crystal composed principally of carbonate of lime and carbonate of magnesia, but containing a sufficient quantity of the carbonate of iron to render it magnetic. Suspended in the magnetic field, the optic axis sets from pole to pole.

Dolomite.—In this crystal a portion of the lime is replaced by protoxide of iron and protoxide of manganese, which ingredients render it magnetic. The optic axis sets from pole to pole.

Carbonate of Iron.—In the cases cited, the substitution of iron for calcium was partial; in the case now before us the substitution is complete. This crystal differs in nothing, save in the energy of its action, from the magnetic crystals already described. If a full crystal be hung between the poles, with its optic axis horizontal, on closing the circuit and sending a current round the magnet, the axis sets strongly in the line joining the poles, vibrates through it quickly for a time, and finally comes to rest there. If a thin rhombus be cloven from the crystal and

suspended from one of its obtuse angles with its parallel faces vertical, it will set itself exactly equatorial. In this case it is easy to see that the horizontal projection of the optic axis, which passes through the obtuse angle of the crystal, stands axial. Hung from its acute angle, the rhombus takes up an oblique position, making a constant angle with the line joining the poles. To this position, if forcibly removed from it, it will invariably return. The position may be either right or left of the axial line; but the angle of obliquity is always the same, being the angle which the optic axis makes with the face of the rhombus. Hung from the obtuse angle the obliquity is nothing—from the acute angle it is a maximum; the rhombus is capable of all degrees of obliquity between these extremes, *the optic axis setting in all cases from pole to pole.*

Oxide of Iron.—The above phenomena are exhibited even in a more striking manner by this crystal. So strong is the directive power that a rhombus, suspended from one of its obtuse angles, will set itself strongly equatorial, though its length may be fifteen or twenty times its breadth.

What is the conclusion to be drawn from these experiments? We have first of all a diamagnetic crystal of pure carbonate of lime, which sets its optic axis equatorial. On substituting for a portion of the lime a quantity of protoxide of iron sufficient to render the crystal weakly magnetic, we find the position of the axis at once reversed. Replacing a still further quantity of the diamagnetic lime by a magnetic constituent, we find the action stronger, the force with which the optic axis takes up the axial position increasing as the magnetic constituents increase. These experiments appear to be irreconcilable with the statement, that the position of the optic axis is independent of the magnetism or diamagnetism of the mass of the crystal.

Turning now to crystals possessing two optic axes, we find the law of Plücker equally untenable.

Dichroite.—This crystal, as is well known, receives its name from its ability to transmit light of two different colours. The specimen examined by us is a cube. In the direction of the '*crystallographic*' axis, which coincides with the '*middle line*,' the light transmitted is yellowish; through the other four sides of the cube it is a deep blue. Suspended with the middle line horizontal, whatever be the position of that line before

closing the circuit, the instant the magnetic force is developed it turns with surprising energy into the axial position and becomes fixed there. According to the law, however, the middle line should stand equatorial, for the crystal is negative.*

Sulphate of Baryta (*Heavy spar*).—The form of this crystal is a prism whose base is a rhombus, the four sides being perpendicular to the base. It cleaves parallel to the sides and base. Suspended between the poles, with the axis of the prism vertical, on exciting the magnet, though the crystal is diamagnetic, the long diagonal sets itself axial. It agrees thus far with the carbonate of lime. Suspended from the acute angle formed by two sides of the prism, on closing the circuit the axis sets parallel to the line joining the poles, and remains there as long as the force is present. Suspending the crystal from its obtuse angle, the axis being still horizontal, on closing the circuit the axis sets itself equatorial. A plane perpendicular to the rhombic base, and passing through the long diagonal, contains the two optic axes, which are inclined to each other at an angle of 38°. The middle line bisecting this angle is parallel to the axis of the prism, and hence stands axial or equatorial, according as the prism is suspended from its acute or its obtuse angle. The position of the middle line is therefore a function of the point of suspension, varying as it varies; at one time supporting the law of Plücker, and at another time contradicting it. Heavy spar is positive.

Sulphate of Strontia (*Cœlestine*).—This is also a positive crystal, its form being precisely that of heavy spar; the only difference is this, that, in Cœlestine, the optic axes enclose an angle of 50° instead of 38°. The corroboration and contradiction exhibited by heavy spar are exhibited here also.

Sulphate of Zinc.—Suppose the crystalline prism to be hung from its end, and the line which stands equatorial when the magnet is excited carefully marked. A plate taken from the crystal parallel to this line, and to the axis of the prism, displays, on examination with polarised light, the ring systems surrounding the ends of the two optic axes. The middle line, which bisects the acute angle enclosed by these axes, is perpendicular to the

* Brewster's list.

surface of the plate, and therefore stands axial. It ought, however, to stand equatorial, for the crystal is negative.

Sulphate of Magnesia.—Suspending the crystalline prism from its end, and, following the method applied in the case of sulphate of zinc, we discover the ring systems and the position of the middle line. This line stands axial, but the crystal is nevertheless negative.

Topaz.—This being one of the crystals pronounced by M. Plücker as peculiarly suited to the illustration of his new law, it is perhaps on that account deserving of more than ordinary attention. In the letter to Mr. Faraday, before alluded to, M. Plücker writes:—

'The crystals most fitted to give the evidence of this law are *diopside* (a positive crystal), *cyanite*, *topaz* (both negative), and others crystallising in a similar way. In these crystals the line (A), bisecting the acute angles made by the two optic axes, is neither perpendicular nor parallel to the axis (B) of the prism. Such a prism, suspended horizontally, will point neither axially nor equatorially, but will take always a fixed intermediate direction. This direction will continually change if the prism be turned round its own axis (B). It may be proved by a simple geometrical construction, which shows that during one revolution of the prism round its axis (B), this axis, without passing out of two fixed limits C and D, will go through all intermediate positions. The directions C and D, where the crystal returns, makes, *either* with the line joining the two poles, *or* with the line perpendicular to it, on both sides of these lines, angles equal to the angle included by A and B; the first being the case if the crystal be a *positive* one, the last if a *negative* one. Thence it follows that if the crystal, by any kind of horizontal suspension, should point to the poles of a magnet, it is a *positive* one; if it should point equatorially, it is a *negative* one.'*

In experimenting with this crystal, we have found the greatest care to be necessary. Its diamagnetic force is so weak, that the slightest local impurity, contracted by handling or otherwise, is sufficient to derange its action. The crystals as they come from the mineralogist are unfit for exact experiment.

* Phil. Mag., vol. xxxiv. p. 450.

We have found it necessary to boil those we have used in muriatic acid, and to scour them afterwards with fine white sand, reduced to powder in a mortar. These precautions taken, we have been unable to obtain the results described by M. Plücker. We have examined five specimens of topaz from Saxony, the axial dimension of some of them exceeding the dimension perpendicular thereto by one-half; the axis, notwithstanding, stands in all cases from pole to pole. Two specimens of Brazilian topaz, the one an amber colour, the other almost as clear as distilled water, gave the same results; the axes of the crystals stand from pole to pole, and turning round makes no difference. On a first examination, some of the crystals exhibited an action similar to that described by M. Plücker; after boiling and scouring, these irregularities disappeared, and the axes one and all stood axial.

One crystal in particular caused us considerable embarrassment. Its action was irregular, and the irregularity remained after the adoption of the methods described to ensure purity. On examination, however, a splinter from one of its sides was found to be attracted, a splinter from the side opposite was found to be repelled. To the naked eye the crystal appeared clean and clear. On examination, however, under a powerful microscope, the side of the crystal from which the magnetic splinter was taken was found dotted with small black particles imbedded in its mass; the other side of the crystal was perfectly transparent. On cleaving away the impurities, the irregularity vanished, and the crystal stood as the others.

In the letter quoted, diopside is pronounced by M. Plücker to be a positive crystal. On examination with circular polarized light, as recommended by Dove,* we find the crystal to be negative. The same method pronounces topaz positive, instead of negative, as affirmed by M. Plücker. The specimens we have examined in this way are from Brazil and Saxony. Aberdeen topaz we have not examined, but it also is classed by Brewster among positive crystals. The obliquity of the middle line of topaz does not exist in the specimens which have come under our notice; it is exactly perpendicular to the planes of principal cleavage, and consequently exactly parallel to the axis of

* Poggendorff's *Annalen*, vol. xl. pp. 457, 482.

the prism. This agrees with the results of Brewster, who found the optic axes to be 'equally inclined to the plane of cleavage.'*

In experimenting with weak diamagnetic crystals, the greater the number of examples the better; as, if local impurity be present, it is thus more liable to detection. Our results with heavy spar have been confirmed by ten different crystals; with cœlestine, by five; and with topaz, as has been stated, by seven. The suspending fibre, in these and similar instances, was a foot in length and $\frac{1}{2500}$ of an inch thick, or about one-eighth of the diameter of a human hair.

Sugar.—It is well known that this crystal forms a prism with six sides, two of which are generally very prominent, the principal cleavage being parallel to these two, and to the wedge-like edge which runs along the end of the prism. The plane of the optic axes is perpendicular to the axis of the prism, and their ends may be found by cutting out a plate parallel to that axis, and inclined to the principal cleavage at an angle of about 20°. Such a plate exhibits both ring systems symmetrically, while a plate parallel to the principal cleavage exhibits one system only. Suspended between the excited poles, with the axis of the prism horizontal, and the principal cleavage vertical, the plane of the optic axes sets axial. According to the law of M. Plücker, it ought to stand equatorial, for the crystal is negative.

Rock-crystal (*Quartz*).—This crystal has undergone more than one examination by M. Plücker, its deportment being, 'contrary to all expectation,' very weak—a result, it may be remarked, difficult of explanation on the hypothesis of an 'optic axis force.' M. Plücker's first experiments with this crystal were apparently made with great exactitude, the crystal being reduced to a spherical shape, and the influence of mere form thus annulled. These experiments proved the optic axis to be repelled. Later researches, however, induced the philosopher to alter his opinion, and accordingly, in his last memoir,† we find quartz ranked with those crystals whose optic axes are attracted, with the remark 'weak' added parenthetically. We have not been able to obtain this deportment.

* Lardner's Encyclopædia, Optics, p. 204.
† Poggendorff's *Annalen*, vol. lxxviii. p. 428.

After the washing and scouring process, the finest and most transparent crystals we could procure confirmed the first experiments of M. Plücker, and therefore contradict the new modification of his law. It is almost incredible how slight an impurity is sufficient to disturb the action of this crystal. A specimen with smaller crystals attached to it, or growing through it, is suspicious and ought to be rejected. Clear isolated crystals are alone suitable. We must remark that a fine cube, with faces half an inch square, suspended with the optic axis horizontal, showed no directive action; either one or the other of the diagonals set itself from pole to pole, though the axis ran parallel to four of the faces.

As far as it has been practicable, we have cut and cloven, and examined the optical properties of the crystals which have passed through our hands ourselves, testing, in every possible case, the results of others by actual experiment. Most of the crystals in Brewster's list have been gone through in this way. Iceland spar, quartz, mica, arragonite, diopside, lepidolite, topaz, saltpetre, sugar, sulphate of zinc, sulphate of magnesia, and others have been examined and verified. In two cases, however, our results differed from the list, these being sulphate of nickel and borax. A prism of sulphate of nickel was suspended from its end between the poles; on exciting the magnet it took up a determinate position. When it came to rest, a line parallel to the magnetic axis was marked thereon, and a plate taken from the crystal parallel to this line and to the axis of the prism. Such a plate, ground thin, exhibited in the polariscope a pair of very beautiful ring systems. The ring systems of borax were found in a similar manner. The middle line, therefore, in both cases stood equatorial, and, according to the list, would contradict the law of M. Plücker, for both are there set down as *positive*. A careful examination with circular polarised light led us to the opposite conclusion. We thought it worth while to send specimens of each to Berlin, so as to have them examined by Professor Dove, the author of the method by which we examined them. The crystals have been returned to us with a note certifying that they are *negative*, thus confirming our observations. This certificate has reached us in the form of a private note, but we believe Professor Dove will not charge us

with imprudence for thus availing ourselves of the high authority of his opinion.

Yellow Ferrocyanide of Potassium.—This crystal does not stand in the list of Brewster, and we have sought for it in other lists in vain. In one German work on physics we find *Blutlaugensalz* set down as a negative crystal with one optic axis, but whether the red or yellow salt is meant, the author does not explain. We have examined the crystal ourselves, and find it positive with two optic axes. The middle line stands perpendicular to the principal cleavage. Suspended with this line horizontal, on closing the circuit it sets itself equatorial. Another exception to the law under consideration is here exhibited.

M. Plücker recommends the magnet as a practical means of determining whether a crystal is positive or negative; this method being attended with the peculiar advantage that it can be applied in the case of opaque crystals, where all the ordinary methods fail. We find accordingly, in his last memoir on this subject, metallic and other opaque crystals with optical properties attributed to them. Antimony is negative with one optic axis; bismuth and arsenic are positive with one optic axis. The foregoing experiments demonstrate the insecurity of the basis on which this classification rests.

By looking back upon the results described, it will be seen that we have drawn from each respective class of crystals one or more examples which disobey the law of M. Plücker. Of positive crystals with one axis, we have quartz; of positive crystals with two axes, we have heavy spar, cœlestine and ferrocyanide of potassium. Of negative crystals with one axis, we have carbonate of lime and iron, and several others; of negative crystals with two axes, we have dichroite, sugar, sulphate of zinc, and sulphate of magnesia. It is due, however, to M. Plücker to state that, in a considerable number of cases, we have found his law confirmed. Tourmaline, idocrase, beryl, Iceland spar, saltpetre, arragonite, and many others, all confirm it. Singularly enough, these are the very crystals with which M. Plücker has experimented. It is therefore not to be wondered at, that he should be led by such a mass of concurring evidence to pronounce his law general. Had his experiments embraced a sufficient number of cases, they would

doubtless have led him to the same conclusion to which ours have conducted us.

Mr. Faraday has devoted considerable time to the investigation of this intricate subject. His most notable experiments are those with *bismuth*, *antimony*, *arsenic*, *sulphate of iron*, and *sulphate of nickel*, which experiments we have carefully repeated.

Bismuth.—Crystals of bismuth we have ourselves prepared, by melting the metal in a Hessian crucible, placed within a larger one and surrounded by fine sand. In this state it was allowed to cool slowly, until a thin crust gathered on the surface. At this point the crust was pierced, and the molten metal underneath poured out, thus leaving the complete crystals clustering round the sides and bottom. Our experiments with these crystals corroborate, to the letter, those so minutely described by Mr. Faraday in the Bakerian Lecture, delivered before the Royal Society in 1849.*

Arsenic.—Our arsenic we procured at the druggists'. It is well known that this metal is usually obtained by the sublimation of its ore, the vapour being condensed in suitable receivers, where it is deposited in a crystalline form. There is a difference of opinion between Mr. Faraday and M. Plücker as regards this metal; the former holding it for diamagnetic, the latter for magnetic. Several specimens, obtained from different druggists, corroborated the view of M. Plücker. They were all *magnetic*.

About half an ounce of the metal was introduced into a glass tube, closed at one end and open at the other. About five inches of the tube, near the open end, was crammed full of copper turnings, and the open end introduced through a small aperture into the strong draft of a flue from a heated oven. The portion of the tube containing the copper turnings was heated to redness, and by degrees the oxygen within the tube was absorbed. The arsenic at the other end was then heated and sublimed. After some time the vapour was allowed to condense slowly, and a metallic deposit was the consequence—the arsenic thus obtained was *diamagnetic*. The deportment of the crystal is described by Mr. Faraday in the place referred to.

* Phil. Trans., 1849, p. 1.

Antimony.—A difference of opinion exists with regard to the action of this crystal also. Referring to the deportment assigned to it by Mr. Faraday, M. Plücker writes, 'to my astonishment, however, antimony behaved in a manner directly the reverse. While on the one side a prism of bismuth, whose principal cleavage coincided with the base of the prism, set itself *axial*; and on the other side a plate of arsenic, which, on account of its magnetism, ought to stand *axial*, set itself *equatorial*; a plate of antimony deviated completely from this deportment, and although the mass was strongly diamagnetic, set itself decidedly *axial*.'

M. Plücker's results differ from those of Mr. Faraday in two particulars; firstly, a plate of antimony, similar to that described by the German philosopher, is found by Mr. Faraday to stand *equatorial* instead of *axial*; secondly, the following phenomena, observed by Mr. Faraday, appear not to have exhibited themselves in M. Plücker's experiments:—'On the development of the magnetic force, the crystal went up to its position slowly, and pointed as with a dead set. Other crystals did the same imperfectly; and others again made one or perhaps two vibrations, but all appeared as if they were moving in a thick fluid, and were, in that respect, utterly unlike bismuth, in the freedom and mobility with which it vibrated. If the crystalline mass was revolving when the magnetic force was excited, it suddenly stopped, and was caught in a position which might, as was found by experience, be any position. The arrest was followed by a revulsive action on the discontinuance of the electric current.' *

In most of the specimens examined by us these phenomena were also absent, and the results of M. Plücker presented themselves. Three specimens, however, behaved exactly in the manner described by Mr. Faraday, exhibiting a singular inertness when the magnetic force was present, and a revulsion from the poles on breaking the circuit. To ascertain, if possible, the cause of this difference, we dissolved an example of each class in muriatic acid, precipitated the antimony with distilled water, and tested the clear filtrate with ferrocyanide of potassium. The specimen which agreed with M. Plücker exhibited a faint bluish tint, characteristic of the presence of

* Phil. Trans., 1849, p. 14. For an explanation see Phil. Mag., vol. xxviii. p. 460.

iron; that which corroborated Mr. Faraday showed not the slightest trace of this metal. The iron, though thus revealing itself, must have been present in a quantity exceedingly minute, for the antimony was diamagnetic. Whether this has been the cause of the difference between M. Plücker and Mr. Faraday we will not undertake to say; irregular crystalline structure may also have had an influence.

We have here a crowd of examples of crystalline action in the magnetic field, but as yet not a word of explanation. M. Plücker's hypothesis has evidently failed. We now turn to the observations of Mr. Faraday, and shall endeavour to exhibit, in the briefest manner possible, the views of this profound investigator.

After a general description of the action of bismuth between the poles, Mr. Faraday writes:—'The results are, altogether, very different from those produced by diamagnetic action. They are equally distinct from those dependent on ordinary magnetic action. They are also distinct from those discovered and described by Plücker, in his beautiful researches into the relation of the optic axis to magnetic action; for there the force is equatorial, whereas here it is axial. So they appear to present to us a new force, or a new form of force in the molecules of matter, which for convenience' sake, I will conventionally designate by a new word, as the *magne-crystallic* force.' *

'The magne-crystallic force appears to be very clearly distinguished from either the magnetic or diamagnetic forces, in that it causes neither approach nor recession; consisting not in attraction or repulsion, but in its giving a certain determinate position to the mass under its influence, so that a given line in relation to the mass is brought by it into a given relation with the direction of the external magnetic power.' †

The line through the crystal which sets itself with greatest force from pole to pole, is termed by Mr. Faraday the magne-crystallic axis of the crystal. He proves by experiment that bismuth has exactly the same amount of repulsion whether this axis be *parallel* or *transverse* to the lines of magnetic force acting on it.‡

* Phil. Trans., 1849, p. 4. † Phil. Trans., 1849, p. 22.
‡ Faraday corrected this. See Introduction, xiv.; also p. 55.

'In other experiments a vertical axis was constructed of cocoon silk, and the body to be examined was attached to it at right angles as radius; a prismatic crystal of sulphate of iron, for instance, whose length was four times its breadth, was fixed on the axis with its length as radius and its magne-crystallic axis horizontal, and therefore as tangent; then, when this crystal was at rest under the torsion force of the silken axis, an electro-magnetic pole was so placed that the axial line of magnetic force should be, when exerted, oblique to both the length and the magne-crystallic axis of the crystal; and the consequence was, that, when the electric current circulated round the magnet, the crystal actually *receded* from the magnet under the influence of the force, which tended to place the magne-crystallic axis and the magnetic axis parallel. Employing a crystal or plate of bismuth, that body could be made to *approach* the magnetic pole under the influence of the magne-crystallic force; and this force is so strong as to counteract either the tendency of the magnetic body to approach, or of the diamagnetic body to retreat, when it is exerted in the contrary direction.' Hence Mr. Faraday concludes that it is neither attraction nor repulsion which causes the set or determines the final position of a magne-crystallic body.*

'As made manifest by the phenomena, the magne-crystallic force is a force acting at a distance, for the crystal is moved by the magnet at a distance, and the crystal can also move the magnet at a distance.' Mr. Faraday obtained the latter result by converting a steel bodkin into a magnet, and suspending it freely in the neighbourhood of the crystal. The tendency of the needle was always to place itself parallel to the magne-crystallic axis.

Crystals of bismuth lost their power of pointing at the moment the metal began to fuse into drops over a spirit-lamp or in an oil-bath. 'Crystals of antimony lost their magne-crystallic power below a dull red heat, and just as they were softening so as to take the impression of the copper loop in which they were hung.' Iceland spar and tourmaline, on the contrary, on being raised to the highest temperature which a spirit-lamp could give, underwent no diminution of force; they pointed equally well as before.

* Phil. Mag. vol. xxxiv. p. 77.

Mr. Faraday finally divides the forces belonging to crystals into two classes—*inherent* and *induced.* An example of the former is the force by which a crystal modifies a ray of light which passes through its mass; the second is developed exclusively by magnetic power. To this latter, as distinct from the other, Mr. Faraday has given the name *magneto-crystallic.* To account for crystalline action in the magnetic field, we have, therefore, the existence of *three new forces* assumed :—the *optic axis* force, the *magne-crystallic* force, and the *magneto-crystallic* force.

With regard to the experimental portion of Mr. Faraday's labours on this subject, we have only to express our admiration of the perfect exactitude with which the results are given. It appears to us, however, a matter of exceeding difficulty to obtain a clear notion of any such force as he has described; that is to say, a force proceeding from the pole of a magnet, and capable of producing such motions in the magnetic field, and yet neither attractive nor repulsive.

That a crystal of bismuth should approach the magnetic pole, and that a crystal of sulphate of iron should recede therefrom, appears, at first sight, anomalous, but certainly not more so than other phenomena connected with one of Mr. Faraday's most celebrated discoveries, and explained in a beautiful and satisfactory manner by himself.

If we hang a penny from its edge in the magnetic field, and so arrange the suspending thread that the coin, before the magnetic power is developed, shall make an angle of 45°, or thereabouts, with the line joining the poles; then, on closing the circuit, and sending a current round the magnet, the coin will suddenly turn, as if it made an effort to set itself from pole to pole; and if its position beforehand be nearly axial, this effort will be sufficient to set it *exactly* so; the penny thus behaving, to all appearance, as if it were attracted by the poles.

The real cause of this however is *repulsion.* During the development of magnetic power, an electric current is aroused in the copper coin, which circulates round the coin in a direction opposite to that of the current which passes from the battery round the coils of the magnet. The effect of this induced current is to create a polar axis in the copper; and when the direction of the current is considered, it is easy to see that the north end of this axis must *face* the north pole of the magnet,

and will consequently be repelled. On looking therefore at the penny, apparently attracted as above described, we must, if we would conceive rightly of the matter, withdraw our attention from the coin itself, and fix it on a line passing through its centre, and at right angles to its flat surface; this is the polar axis of the penny, the repulsion of which causes the apparent attraction.

We do not mean to say that any such action as that here described takes place with a bismuth crystal in the magnetic field. The case is cited merely to show that the 'approach' of the bismuth crystal, noticed by Mr. Faraday, *may* be really due to *repulsion*; and the 'recession' of the sulphate of iron really due to *attraction*.

Our meaning will perhaps unfold itself more clearly as we proceed. If we take a slice of apple, about the same size as the penny, but somewhat thicker, and pierce it through with short bits of iron wire, in a direction perpendicular to its flat surface, such a disc, suspended in the magnetic field, will, on the evolution of the magnetic force, recede from the poles and set its horizontal diameter strongly equatorial; *not* by repulsion, but by the attraction of the iron wires passing through it. If, instead of iron, we use bismuth wire, the disc, on exciting the magnet, will turn into the axial position; *not* by attraction, but by the repulsion of the bismuth wires passing through it.

If we suppose the slice of apple to be replaced by a little cake made of a mixture of flour and iron filings, the bits of wire running through this will assert their predominance as before; for though the whole is strongly magnetic, the superior energy of action along the wire will determine the position of the mass. If the bismuth wire, instead of piercing the apple, pierce a little cake made of flour and bismuth filings, the cake will stand between the poles as the apple stood; for though the whole is diamagnetic, the stronger action along the wire will be the ruling agency as regards position.

Is it not possible to conceive an arrangement among the particles of a magnetic or diamagnetic crystal, capable of producing a visible result similar to that here described? If, in a magnetic or diamagnetic mass, two directions exist, in one of which the contact of the particles is closer than in the other, may we not fairly conclude that the strongest exhibition of force

will be in the former line, which therefore will signalise itself between the poles, in a manner similar to the bismuth or iron wire? The case seems analogous to that of good and bad conductors in electricity. This fluid will not quit the good conductor to go to the bad. The powder magazine is safe, because the fluid prefers the iron rod to any other path. As regards magnetism, different directions, *through the same body*, may represent these good and bad conductors; *the line of preference being that of closest contact among the material particles*. [The illustration is not a good one.—J. T. 1870.]

If analogic proof be of any value, we have it here of the very strongest description. For example:—bismuth is a brittle metal, and can readily be reduced to a fine powder in a mortar. Let a teaspoonful of the powdered metal be wetted with gum-water, kneaded into a paste, and made into a little roll, say an inch long and a quarter of an inch across. Hung between the excited poles, it will set itself like a little bar of bismuth—equatorial. Place the roll, protected by bits of pasteboard, within the jaws of a vice, squeeze it flat, and suspend the plate thus formed between the poles. On exciting the magnet the plate will turn, with the energy of a magnetic substance, into the axial position, though its length may be ten times its breadth.

Pound a piece of carbonate of iron into fine powder, and form it into a roll in the manner described. Hung between the excited poles, it will stand as an ordinary magnetic substance—axial. Squeeze it in the vice and suspend it edgeways, its position will be immediately reversed. On the development of the magnetic force, the plate thus formed will recoil from the poles, *as if violently repelled*, and take up the equatorial position.

We have here 'approach' and 'recession,' but the cause is evident. The line of closest contact is perpendicular in each case to the surface of the plate—a consequence of the pressure which the particles have undergone in this direction; and this perpendicular stands axial or equatorial according as the plate is magnetic or diamagnetic. We have here a 'directive force,' but it is attraction or repulsion modified. May not that which has been here effected by artificial means occur naturally? *Must* it not actually occur in most instances? for, where perfect homogeneity of mass does not exist, there will always be a preference shown by the forces for some particular direction. This

election of a certain line is therefore the rule and not the exception. It will assist both the reader and us if we give this line a name; we therefore propose to call it *the line of elective polarity*.* In magnetic bodies this line will stand axial, in diamagnetic equatorial.

'The relation of the magne-crystallic force,' says Mr. Faraday, 'to the magnetic field is axial and not equatorial.' This he considers to be proved by the following considerations:—Suppose a crystal of bismuth so suspended that it sets with its *maximum* degree of force, then if the point of suspension be moved 90° in the axial plane, so that the line which in the last case stood horizontal and axial, may now hang vertical, then the action is a *minimum*: now, contends Mr. Faraday, if the force were equatorial this change in the axial plane ought not to have affected it; that is to say, if the force act at right angles to the axial plane, it is all the same which point of the plane is chosen as the point of suspension.

This seems a fair conclusion; but the other is just as fair—that, if the force be axial, a change of the point of suspension in the equatorial plane cannot disturb it. In sulphate of nickel, Mr. Faraday finds the line of maximum force to be parallel to the axis of the prism. Whatever, therefore, be the point of suspension in the plane perpendicular to the axis, the action ought to be the same. On examining this crystal it will probably be found that two opposite corners of the parallelopiped are a little flattened. Let the prism be hung with its axis horizontal and this *flattening* vertical, and after the evolution of the magnetic force let the oscillations of the prism be counted. Move the point of suspension 90° in the equatorial plane, so that the flattening shall be horizontal, and again count the oscillations. The numbers expressing the oscillations in both cases will be very different. The former will be a *maximum*, the latter a *minimum*. But if the force be axial this is impossible, therefore the force is not axial.

Whatever be the degree of conclusiveness which attaches itself to the reasoning of Mr. Faraday drawn from bismuth; precisely the same degree attaches to the above drawn from sulphate of nickel. The conclusions are equal and opposite, and hence destroy each other. It will probably be found that

* The principal axis of magnetic induction.—J. T. 1870.

the reasoning in both cases is entirely correct; that the force is neither axial nor equatorial, in the sense in which these terms are used.

A number of thin plates, each about half an inch square, were cut from almond kernels, with an ivory blade, parallel to the cleft which divides the kernel into two lobes. These were laid one upon the other, with an interval of strong gum between, until a cube was obtained. A few minutes in the sunshine sufficed to render the cube dry enough for experiment. Hung between the poles, with the line perpendicular to the layers horizontal, on exciting the magnet this line turned and set itself parallel to the magnetic resultant passing through the mass. The action here was a *maximum*. Turning the cube round 90° in the axial plane, there was scarcely any directive action. If the word 'crystal' be substituted for 'cube' in the description of this deportment, every syllable of it is applicable to the case of bismuth; and if the deportment of the crystal warrant the conclusion that the force is axial, the deportment of the cube warrants the same conclusion. *Is* the force axial in the case of the cube? *Is* the position of the line perpendicular to its layers due to the 'tendency' of that line to set itself parallel to the magnetic resultant? The kernel is strongly diamagnetic, and the position of the perpendicular is evidently a secondary result, brought about by the repulsion of the layers. *Is it not then possible, that the approach of the magne-crystallic axis, in bismuth, to the magnetic resultant, is really due to the repulsion of the planes of cleavage?*

But here the experiment with the silken axis meets us; which showed that so far from attraction being the cause of action in a magnetic crystal, there was actual *recession*; and so far from repulsion being the cause in a diamagnetic crystal, there was actual *approach*. This objection it is our duty to answer.

A model was constructed of powdered carbonate of iron, about 0·3 of an inch long and 0·1 in thickness, and, by attention to compression, it was arranged that the line of elective polarity through the model was perpendicular to its length. Hanging a weight from one end of a fibre of cocoon silk a vertical axis was obtained; a bit of card was then slit and fitted on to the axis, so that when the model was laid on one side,

the card stood like a little horizontal table in the middle of the magnetic field. The length of the model extended from the central axis to the edge of the card, so that when the mass swung round, its line of elective polarity was tangent to the circle described.

When the model was made to stand between the flat-faced poles obliquely, the moment the magnet was excited it moved, tending to set its length equatorial and its line of elective polarity parallel to the lines of magnetic force. In this experiment the model of carbonate of iron, though a magnetic body and strongly attracted by such a magnet as that used, actually *receded* from the magnetic pole.

If, instead of the model of carbonate of iron, we substitute a crystal of sulphate of iron, we have Mr. Faraday's experiment instituted to prove the absence of attraction or repulsion. The dimensions are his dimensions, the arrangement is his arrangement, and the deportment is the exact deportment which he has observed. We have copied his very words, these words being perfectly descriptive of the action of the model. If, then, the experiment be 'a striking proof that the effect is not due to attraction or repulsion' in the one case, it must also be such in the other case; but the great experimenter will, we imagine, hardly push his principles so far. He will, we doubt not, be ready to admit, that it is more probable that a line of elective polarity exists in the crystal, than that a magne-crystallic axis exists in the model.*

By a similar proceeding, using bismuth powder instead of carbonate of iron, the action of Mr. Faraday's plate of bismuth may be exactly imitated. The objection to the conclusion, that the approach of the magne-crystallic axis, in bismuth, to the magnetic resultant, is due to the repulsion of the planes of cleavage, is thus, we conceive, fairly met.

Let us look a little further into the nature of this magne-crystallic force, which, as is stated, is neither attraction nor repulsion, but gives position only. The magne-crystallic axis, says Mr. Faraday, *tends* to place itself parallel to the magnetic resultant passing through the crystal; and in the case of a bis-

* The term magne-crystallic axis may with propriety be retained, even should our views prove correct; but then it must be regarded as a subdenomination of the line of elective polarity.

muth plate, the recession from the pole and the taking up of the equatorial position is not due to repulsion, but to the *endeavour* the bismuth makes to establish the parallelism before-mentioned. Leaving attraction and repulsion out of the question, we find it extremely difficult to affix a definite meaning to the words 'tends' and 'endeavour.' 'The force is due,' says Mr. Faraday, 'to that power of the particles which makes them cohere in regular order, and gives the mass its crystalline aggregation, which we call at times the attraction of aggregation, and so often speak of as acting at insensible distances.' We are not sure that we fully grasp the meaning of the philosopher in the present instance; for the difficulty of supposing that what is here called the attraction of aggregation, considered apart from magnetic attraction or repulsion, can possibly cause the rotation of the *entire mass* round an axis, and the taking up of a fixed position by the mass, with regard to surrounding objects, appears to us insurmountable. We have endeavoured to illustrate the matter, to our own minds, by the action of a piece of leather brought near a red-hot coal. The leather will curl and motion will be caused, without the intervention of either attraction or repulsion, in the present sense of these terms; but this motion exhibits itself in *an alteration of shape*, which is not at all the case with the crystal.* Even if the direct attraction or repulsion of the poles be rejected, we do not see how the expressed relation between the magne-crystallic axis and the direction of the magnetic resultant is possible, without including the idea of *lateral attraction* between these lines, and consequently of the mass associated with the former. In the case of flat poles, the magnetic resultant lies in a straight line from pole to pole across the magnetic field. Let us suppose, at any given moment, this line and the magne-crystallic axis of a properly suspended crystal to cross each other at an oblique angle; let the crystal be forgotten for a moment, and the attention fixed on those two lines. Let us suppose the former line fixed, and the latter free to rotate, the point of intersection being regarded as a kind of pivot round which it can turn. On the evolution of the magnetic force, the magne-crystallic axis *will* turn and set itself alongside the magnetic resultant. The matter may be rendered very clear by taking a pair of scissors,

* The subsequent reasoning of this paragraph might be omitted.—J. T. 1870.

partly open, in the hand, holding one side fast, and then closing them. The two lines close in a manner exactly similar; and all that is required to make the illustration perfect, is to suppose this power of closing suddenly developed *in the scissors themselves.* How should we name a power resident in the scissors and capable of thus drawing the blades together? It may be called a 'tendency,' or an 'endeavour,' but the word *attraction* seems to be as suitable as either.

The symmetry of crystalline arrrngement is annihilated by reducing the mass to powder. 'That force among the particles which makes them cohere in regular order' is here ineffective. The magne-crystallic force, in short, is reduced to nothing, but we have the same results. If, then, the principle of elective polarity, the mere modification of magnetism or diamagnetism by mechanical arrangement, be sufficient to explain the entire series of crystalline phenomena in the magnetic field, why assume the existence of this new force, the very conception of which is attended with so many difficulties? *

APPLICATION OF THE PRINCIPLE OF ELECTIVE POLARITY TO CRYSTALS.

We shall now endeavour to apply the general principle of elective polarity to the case of crystals. This principle may be briefly enunciated as follows:—

If the arrangement of the component particles of any body be such as to present different degrees of proximity in different directions, then the line of closest proximity, other circumstances being equal, will be that chosen by the respective forces for the exhibition of their greatest energy. If the mass be magnetic, this line will stand axial; if diamagnetic, equatorial.

From this point of view, the deportment of the two classes of crystals, represented by Iceland spar and carbonate of iron, presents no difficulty. This crystalline form is the same; and as to the arrangement of the particles, what is true of one will be true of the other. Supposing, then, the line of closest proximity to coincide with the optic axis; this line, according to the principle expressed, will stand axial or equatorial, according as

* 'Perhaps,' says Mr. Faraday, in a short note referring to 'the strange and striking character' of these force, 'these points may find their explication hereafter in the action of contiguous particles.'

the mass is magnetic or diamagnetic, which is precisely what the experiments with these crystals exhibit.

Analogy, as we have seen, justifies the assumption here made. It will, however, be of interest to inquire, whether any discoverable circumstance connected with crystalline structure exists, upon which the difference of proximity depends; and, knowing which, we can pronounce with tolerable certainty, as to the position which the crystal will take up in the magnetic field.

The following experiments will perhaps suggest a reply.

If a prism of sulphate of magnesia be suspended between the poles with its axis horizontal, on exciting the magnet the axis will take up the equatorial position. This is not entirely due to the form of the crystal; for even when its axial dimension is shortest, the axis will assert the equatorial position; thus behaving like a magnetic body, setting its longest dimension from pole to pole.

Suspended from its end with its axis vertical, the prism will take up a determinate oblique position. When the crystal has come to rest, let that line through the mass which stands exactly equatorial be carefully marked. Lay a knife-edge along this line, and press it in the direction of the axis. The crystal will split before the pressure, disclosing shining surfaces of cleavage. This is the only cleavage the crystal possesses, and it stands equatorial.

Sulphate of zinc is of the same form as sulphate of magnesia, and its cleavage is discoverable by a process exactly similar to that just described. Both crystals set their planes of cleavage equatorial. Both are diamagnetic.

Let us now examine a magnetic crystal of similar form. Sulphate of nickel is, perhaps, as good an example as we can choose. Suspended in the magnetic field with its axis horizontal, on exciting the magnet the axis will set itself from pole to pole; and this position will be persisted in, even when the axial dimension is shortest. Suspended from its end, the crystalline prism will take up an oblique position with considerable energy. When the crystal thus suspended has come to rest, mark the line along its end which stands *axial*. Let a knife-edge be laid on this line, and pressed in a direction parallel to the axis of the prism. The crystal will yield before the edge, and discover a perfectly clean plane of cleavage.

These facts are suggestive. The crystals here experimented with are of the same outward form; each has but one cleavage; and the position of this cleavage, with regard to the form of the crystal, is the same in all. The magnetic force, however, at once discovers a difference of action. *The cleavages of the diamagnetic specimens stand equatorial; of the magnetic, axial.*

A cube cut from a prism of scapolite, the axis of the prism being perpendicular to two of the parallel faces of the cube, suspended in the magnetic field, sets itself with the axis of the prism from pole to pole.

A cube of beryl, of the same dimensions, with the axis of the prism from which it was taken also perpendicular to two of the faces, suspended as in the former case, sets itself with the axis equatorial. Both these crystals are magnetic.

The former experiments showed a dissimilarity of action between magnetic and diamagnetic crystals. In the present instances both are magnetic, but still there is a difference; the axis of the one prism stands axial, the axis of the other equatorial. With regard to the explanation of this, the following fact is significant. Scapolite cleaves *parallel* to its axis, while beryl cleaves *perpendicular* to its axis; the cleavages in both cases, therefore, stand axial, thus agreeing with sulphate of nickel. The cleavages hence appear to take up a determinate position, regardless of outward form, and they seem to exercise a ruling power over the deportment of the crystal.

A cube of saltpetre, suspended with the crystallographic axis horizontal, sets itself between the poles with this axis equatorial.

A cube of topaz, suspended with the crystallographic axis horizontal, sets itself with this axis from pole to pole.

We have here a kind of complementary case to the former. Both these crystals are diamagnetic. Saltpetre cleaves parallel to its axis; topaz perpendicular to its axis. The planes of cleavage, therefore, stand in both cases equatorial, thus agreeing with sulphate of zinc and sulphate of magnesia.*

Where do these facts point? A moment's speculation will perhaps be allowed us here. May we not suppose these crystals

* Topaz possesses other cleavages, but for the sake of simplicity we have not introduced them; more especially as they do not appear to vitiate the action of the one introduced, which is by far the most complete.

to be composed of layers indefinitely thin, laid side by side, within the range of cohesion, which holds them together, but yet not in absolute contact? This seems to be no strained idea; for expansion and contraction by heat and cold compel us to assume that the particles of matter in general do not touch each other; that there are unfilled spaces between them. In such crystals as we have described, these spaces may be considered as alternating with the plates which compose the crystal. From this point of view it seems very natural that the magnetic laminæ should set themselves axial, and the diamagnetic equatorial.*

We have a very fine description of sand-paper here. The sand or emery on the surface is magnetic, while the paper itself is comparatively indifferent. By cutting a number of strips of this paper, an inch long and a quarter of an inch wide, and gumming them together so as to form a parallelopiped, we obtain a model of magnetic crystals which cleave parallel to their axis; the layers of sand representing the magnetic crystalline plates, and the paper the intermediate space between two plates. For such a model one position only is possible between the poles, the axial. If, however, the parallelopiped be built up of squares, equal in area to the cross section of the model just described, by laying square upon square until the pile reaches the height of an inch, we obtain a model of those magnetic crystals which cleave perpendicular to their axes. Such a model, although its length is four times its thickness, and the whole strongly magnetic, will, on closing the circuit, recede from the poles as if repelled, and take up the equatorial position with great energy. The deportment of the first model is that of scapolite; of the second, that of beryl. By using a thin layer of bismuth paste instead of the magnetic sand, the deportment of saltpetre and topaz will be accurately imitated.

Our fundamental idea is, that crystals of one cleavage are

* In these speculations we have made use of the commonly received notion of matter. Mr. Faraday, for reasons derived from electric conductibility, and from certain anomalies with regard to the combinations of potassium and other bodies, considers this notion erroneous. Nothing, however, could be easier than to translate the above into a language agreeing with the views of Mr. Faraday. The interval of space between the laminæ would then become intervals of *weaker force*, and the result of our reasoning would be the same as before.

made up of plates indefinitely thin, separated by spaces indefinitely narrow. If, however, we suppose two cleavages existing at right angles to each other, then we must relinquish the notion of plates and substitute that of little parallel bars; for the plates are divided into such by the second cleavage. If we further suppose these bars to be intersected by a cleavage at right angles to their length, then the component crystals will be little cubes, as in the case of rock-salt and others. By thus increasing the cleavages, the original plates may be subdivided indefinitely, the shape of the little component crystal bearing special relation to the position of the planes. It is an inference which follows immediately from our way of viewing the subject, that if the crystal have several planes of cleavage, but all parallel to the same straight line, this line, in the case of magnetic crystals, will stand axial; in the case of diamagnetic, equatorial. It also follows, that in the so-called regular crystals, in rock-salt, for instance, the cleavages annul each other, and, consequently no directive power will be exhibited, which is actually the case.

Everything which tends to destroy the cleavages tends also to destroy the directive power; and here the temperature experiments of Mr. Faraday receive at once their solution. Crystals of bismuth and antimony lose their directive power *just as they melt*, for at this particular instant the cleavages disappear. Iceland spar and tourmaline, on the contrary, retain their directive power, for in their case the cleavages are unaffected. The deportment of rock crystal, whose weakness of action appears to have taken both Mr. Faraday and M. Plücker by surprise—as here the optic axis force, without assigning any reason, has thought proper to absent itself almost totally—follows at once from the homogeneous nature of its mass; it is almost like glass, which possesses no directive power; its cleavages are merely traces of cleavage. If, instead of possessing planes of cleavage, a crystal be composed of a bundle of fibres, the forces may be expected to act with greater energy along the fibre than across it. Anything, in short, that affects the mechanical arrangement of the particles will affect, in a corresponding degree, the line of elective polarity. There are crystals which are both fibrous and have planes of cleavage,

the latter often perpendicular to the fibre ; in this case two opposing arrangements are present, and it is difficult to pronounce beforehand which would predominate.*

The same difficulty extends to crystals possessing several planes of cleavage, oblique to each other, and having no common direction. In many cases, however, the principle may be successfully applied. We shall content ourselves in making use of it to explain the deportment of that class of crystals, of which, as to form, Iceland spar is the type.

For the sake of simplicity, we will commence our demonstration with an exceedingly thin rhombus cloven from this crystal. Looking down upon the flat surface of such a rhombus, what have we before us? It is cleavable parallel to the four sides. Hence our answer must be, 'an indefinite number of smaller rhombuses held symmetrically together by the force of cohesion.' Let us confine our attention, for a moment, to two rows of these rhombuses; the one ranged along the greater diagonal, the other along the less. A moment's consideration will suffice to show, that whatever be the number of small rhombuses supposed to stand upon the long diagonal, precisely the same number must fit along the short one; but in the latter case *they are closer together*. The matter may be rendered very plain by drawing a lozenge on paper, with opposite acute angles of 77°, being those of Iceland spar. Draw two lines, a little apart, parallel to opposite sides of the lozenge, and nearly through its centre; and two others, the same distance apart, parallel to the other two sides of the figure. The original rhombus is thus divided into four smaller ones; two of which stand upon the long diagonal, and two upon the short one, each of the four being separated from its neighbour by an interval which may be considered to represent the interval of cleavage in the crystal. The two which stand upon the long diagonal L, have their acute angles opposite; the two which stand upon the short diagonal, S, have their obtuse angles opposite. The distance between the two former, across the interval of cleavage, is to the distance between the two latter, as L is to S, or as the cosine of 38° 30′ to its sine, or as 4 : 3. We may conceive the size of these rhombuses to decrease till they

* It is probable that the primitive plates themselves have different arrangements of the molecules *along* and *across* them.

become molecular; the above ratio will then appear in the form of a differential quotient, but its value will be unaltered. Here, then, we have along the greater diagonal a row of magnetic or diamagnetic molecules, the distance between each two being represented by the number 4; and along the short diagonal a row of molecules, the distance between each two being represented by the number 3. In the magnetic field, therefore, the short diagonal will be the line of elective polarity; and in magnetic crystals will stand axial, in diamagnetic equatorial, which is precisely the case exhibited by experiment. Thus the apparent anomaly of carbonate of lime setting its long diagonal axial, and carbonate of iron its short diagonal axial, seems to be fully explained; the position of the former line being due, not to any endeavour on its part to stand parallel with the magnetic resultant, but being the simple consequence of the repulsion of the short diagonal.

There is no difficulty in extending the reasoning used above to the case of full crystals. If this be done, it will be seen that the line of closest proximity coincides with the optic axis, which axis, in the magnetic field, will signalise itself accordingly. A remarkable coincidence exists between this view and that expressed by Mitscherlich in his beautiful investigation on the the expansion of crystals by heat.* 'If,' says this gifted philosopher, 'we imagine the repulsive force of the particles increased by the accession of heat, then we must conclude that the line of greatest expansion will be that in which the atoms lie most closely together.' This line of greatest expansion Mitscherlich found, in the case of Iceland spar, to coincide with the optic axis. The same conclusion has thus been arrived at by two modes of reasoning, as different as can well be conceived.

If, then, speculation and experiment concur in pronouncing the line of closest proximity among the particles, to be that in which the magnetic and diamagnetic forces will exhibit themselves with peculiar energy, thus determining the position of the crystalline mass between the poles, we are furnished with a valuable means of ascertaining the relative values of this proximity in different directions through the mass. An *order of contact* might, perhaps, by this means be established, of great interest in a mineralogical point of view. In the case of a

* Poggendorff's *Annalen*, vol. x. p. 138.

right rhombic prism, for example, the long diagonal of the base may denote an order of contact very different from that denoted by the short one; and the line at right angles to the diagonals, that is, the axis of the prism, a contact very different from both. We can compare these lines two at a time. By hanging the short diagonal vertical in the magnetic field, its rotatory power is annulled, and we can compare the long diagonal and the axis. By hanging the long diagonal vertical, we can compare the short diagonal and the axis. By hanging the axis vertical, we can compare the two diagonals. From this point of view the deportment of heavy spar and cœlestine, so utterly irreconcilable with the assumption of an optic axis force, presents no difficulty. If we suppose the proximity along the axis of the prism to be *intermediate* between the proximities along the two diagonals, the action of both crystals follows as a necessary consequence. Suspended from one angle, the axis must stand from pole to pole; from the other angle, it must stand equatorial.

A ball of dough, made from bismuth powder, was placed between two bits of glass and pressed to the thickness of a quarter of an inch. It was then set edgeways between the plates and pressed again, but not so strongly as in the former case. A model of heavy spar was cut from the mass, so that the shorter diagonal of its rhombic base coincided with the line of greatest compression, the axis of the model with the direction of less compression, and the longer diagonal of the base with that direction in which no pressure had been exerted. *When this model was dried and suspended in the magnetic field, there was no recognisable difference between its deportment and that of heavy spar.*

When a crystal cleaves symmetrically in several planes, all parallel to the same straight line, and, at the same time, in a direction perpendicular to this line, then the latter cleavage, if it be more eminent than the former, may be expected to predominate; but when the cleavages are oblique to each other, the united action of several minor cleavages may be such as to overcome the principal one, or so to modify it that its action is not at all the same as that of a cleavage of the same value unintersected by others. A complex action among the particles of the crystal itself may contribute to this result, and possibly in some cases modify even the influence of proximity. If we hang a magnetic body between the poles, it always shows a pre-

ference for edges and corners, and will spring to a *point* much more readily than to a *surface*. Diamagnetic bodies, on the contrary, will recede from edges and corners. *A similar action among the crystalline particles may possibly bring about the modification we have hinted at.*

During this investigation a great number of crystals have passed through our hands, but it is useless to cumber the reader with a recital of them. The number of natural crystals have amounted to nearly one hundred; while through the accustomed kindness of Professor Bunsen, the entire collection of artificial crystals, which his laboratory contains, has been placed at our disposal.*

We now pass over to a brief examination of the basis on which the second law of M. Plücker rests:—the affirmation, namely, that 'the magnetic attraction decreases in a quicker ratio than the repulsion of the optic axis.' The ingenuity of this hypothesis, and its apparent sufficiency to account for the phenomena observed by M. Plücker, are evident. It will be seen, however, that this repulsion arises from quite another cause—a source of error which has run undetected through the entire series of this philosopher's inquiries.

The following experiment is a type of those which led M. Plücker to the above conclusion. A tourmaline crystal 36 millimeters long and 4 millimeters wide was suspended between a pair of pointed movable poles, so that it could barely swing between them. It set its length *axial*. On removing the poles to a distance and again exciting the magnet the crystal set *equatorial*. The same occurred, if the poles were allowed to remain as in the former case, when the crystal was raised above them or sunk beneath them. *Thus, as the crystal was withdrawn from the immediate neighbourhood of the poles it turned gradually round and finally set itself equatorial.*†

A similar action was observed with staurolite, beryl, idocrase, smaragd, and other crystals.

We have repeated these experiments in the manner described, and obtained the same results. A prism of tourmaline three-

* We gladly make use of this opportunity to express our obligation to Dr. Debus, the able assistant in the chemical laboratory.

† Poggendorff's *Annalen*, vol. lxxii. p. 319.

quarters of an inch long and a quarter of an inch across was hung between a pair of poles with conical points, an inch apart. On exciting the magnet the crystal stood axial. When the poles were withdrawn to a distance on the evolution of the force, the crystal stood equatorial. An exceedingly weak current was here used; a single Bunsen's cell being found more than sufficient to produce the result.

According to the theory under consideration, the tourmaline, in the first instance, stood from pole to pole because the magnetism was strong enough to overcome the repulsion of the optic axis. This repulsion, decreasing more slowly than the magnetic attraction, necessarily triumphed when the poles were removed to a sufficient distance. Between a pair of flat poles, however, this same crystal could *never* take up the axial position. On bringing the faces within half an inch of each other, and exciting the magnet by a battery of thirty-two cells, the crystal vibrated between the faces without touching either. The same occurred when one cell, six cells, twelve cells, and twenty cells, respectively, were employed.

If the attraction increases, as stated, more quickly than the hypothetic repulsion, how can the impotence of attraction in the case before us be accounted for? We have here a powerful current, and poles only half an inch apart; power and proximity work together, but their united influence is insufficient to pull the crystal into the axial line. The cause of the phenomena must it seems be sought, not in optic repulsion, but in the manner in which the magnetic force is applied. The crystal is strongly magnetic, and the pointed poles exercise a concentrated *local* action. The *mass* at both ends of the crystal, when in the neighbourhood of the points, is powerfully attracted, while the action on the central parts, on account of their greater distance, is comparatively weak. Between the flat poles, on the contrary, the crystal finds itself, as it were, totally immersed in the magnetic influence; its entire mass is equally affected, and the whole of its directive power developed. The similarity of action between the flat poles and the points, *withdrawn to a distance*, is evident. In the latter case, the force, radiating from the points, has time to diffuse itself, and fastens almost uniformly upon the entire mass of the crystal, thus calling forth, as in the former case, its directive energy; and

the equatorial position is the consequence. The disposition of the lines of force, in the case of points, is readily observed by means of iron filings, strewn on paper and brought over the poles. When the latter are near each other, on exciting the magnet, the filings are gathered in and stretch in a rigid line from point to point; according as the poles are withdrawn, the magnetic curves take a wider range, and at length attain a breadth sufficient to encompass the entire mass of the crystal.*

As the *local attraction* of the mass in the case of magnetic crystals deranges the directive power and overcomes it, so will the *local repulsion* of the mass in diamagnetic crystals. A prism of heavy spar, whose length was twice its breadth, hung from its acute angle, stood between the flat poles axial, between the points equatorial. On making its length and breadth alike, the axis of the prism stood from pole to pole, whether the conical points or flat faces were used. Shortening the axial direction a little more, and suspending the crystal from its obtuse angle, the axis between the flat poles stood equatorial, and, consequently, the longest dimension of the crystal, axial; between the points, owing to the repulsion of the extreme ends, the length stood equatorial. Similar experiments were made with cœlestine and topaz; but all with the same general result.

'I had the advantage,' says Mr. Faraday, 'of verifying Plücker's results under his own personal tuition, in respect of tourmaline, staurolite, red ferrocyanide of potassium, and Iceland spar. Since then, and in reference to the present inquiry, I have carefully examined calcareous spar, as being that one of the bodies which was at the same time free from magnetic action, and so simple in its crystalline relations as to possess but one optic axis.

'When a small rhomboid about 0·3 of an inch in its greatest dimension was suspended with its optic axis horizontal between the pointed poles of the electro-magnet, approximated as closely as they can be to allow free motion, the rhomboid set in the equatorial direction, and the optic axis coincided with the magnetic axis; but if the poles be separated to the distance of a half or three-quarters of an inch, the rhomboid turned through 90° and set with the optic axis in the equato-

* Mr. Faraday has already pointed out 'the great value of a magnetic field of uniform force.'—Phil. Trans., 1849, p. 4.

rial direction, and the greatest length axial. In the first instance the diamagnetic force overcame the optic axis force; in the second the optic axis force was the stronger of the two.'

The foregoing considerations will, we believe, render it very clear that the introduction of this optic axis force is altogether unnecessary; the case being simply one of local repulsion. Mr. Faraday himself found that the crystal between the flat poles could *never* set its optic axis from pole to pole; between the points alone was the turning round of the crystal possible. We have made the experiment. A fine large crystal of Iceland spar, suspended between the near points, set its optic axis from point to point; between the distant points the axis stood equatorial. The crystal was then removed from the magnetic field, placed in an agate mortar and pounded to powder. The powder was dissolved in muriatic acid. From the solution it was precipitated by carbonate of ammonia. The precipitate thus obtained, as is well known, is exactly of the same chemical constitution as the crystal. This precipitate was mixed with gum water and squeezed in one direction. From the mass thus squeezed a model of Iceland spar was made, the line of greatest compression through the model coinciding with that which represented the optic axis. *This model imitated, in every respect, the deportment observed by Mr. Faraday.* Between the near points the optic axis stood from point to point, between the distant points equatorial. It cannot, however, be imagined that the optic axis force survived the pounding, dissolving, and precipitating. Further, this optic axis force is a sword which cuts two ways; if it be assumed repulsive, then the deportment of carbonate of lime and iron is unexplainable; if attractive, it fails in the case of Iceland spar.

It is a remarkable fact, that all those crystals which exhibit this phenomenon of turning round, cleave either perpendicular to their axes or oblique to them, furnishing a resultant which acts in the direction of the perpendicular. Beryl is an example of the former; the crystal just examined, Iceland spar, is an example of the latter. This is exactly what must have been expected. In the case of a magnetic crystal, cleavable parallel to its length alone, there is no reason present why the axial line should ever be forsaken. But if the cleavages be transverse, or oblique, so as to furnish a line of elective

polarity in the transverse direction, two diverse causes come into operation. By virtue of its magnetism, the crystal seeks to set its length axial, as a bit of iron or nickel would do; but in virtue of its *molecular structure*, it seeks to place a line at right angles to its length axial. For the reasons before adduced, if the *near* points be used, the former is triumphant; if the points be distant, the latter predominates.

We noticed in a former paper a description of gutta-percha of a fibrous texture, which, on being suspended between the poles, was found to transmit the magnetic force with peculiar facility along the fibre. A piece was cut from this substance, of exactly the same size as the tourmaline crystal, described at the commencement of this section. The fibre was transverse to the length of the piece. Suspended in the magnetic field, the gutta-percha exhibited all the phenomena of the crystal.

One of the sand-paper models before described is still more characteristic as regards this turning round on the removal of the poles to a distance. We allude to that whose magnetic layers of emery are perpendicular to its length. The deportment of this model, if we except its greater energy, is not to be distinguished from that of a prism of beryl. Between the near points both model and crystal stand axial, between the distant points equatorial, and between the flat poles the deportment, as before described, is exactly the same. The magnetic laminæ of beryl occupy the same position, with regard to its axis, as the magnetic laminæ of the model, with regard to its axis. There is no difference in construction, save in the superior workmanship of nature, and there is no difference at all as regards deportment. Surely these considerations suggest a common origin for the phenomena exhibited by both.

We have the same action in the case of the compressed dough, formed from the powdered carbonate of iron and bismuth. A plate of the former, three-quarters of an inch square and one-tenth of an inch in thickness, stands between the conical poles, brought within an inch of each other, exactly axial; between the same poles, two inches apart, it stands equatorial. A plate of compressed bismuth dough stands, between the near points, equatorial, between the distant points, axial.

Any hypothesis which solves these experiments must embrace crystalline action also; for the results are not to be dis-

tinguished from each other. But in the above cases an optic action is out of the question. With the similarity of structure between beryl and the sand-paper model, above described,—with the complete identity of action which they exhibit, before us, is it necessary, in explanation of that action, to assume the existence of a force which, in the case of the crystal, is all but inconceivable, and in the case of the model is not to be thought of? In his able strictures on the theory of M. Becquerel,* M. Plücker himself affirms, that we have no example of a force which is not associated with ponderable matter. If this be the case as regards the optic axis force, if the attraction and repulsion attributed to it be actually exerted on the mass of the crystal, *how is it to be distinguished from magnetism or diamagnetism?* The assumption of Mr. Faraday appears to be the only refuge here: the abandonment of attraction and repulsion altogether.

In the first section of this memoir it has been proved, by the production of numerous exceptions, that the law of M. Plücker, as newly revised, is untenable. It has also there been shown, that the experiments upon which Mr. Faraday grounds his hypothesis of a purely directive force, are referable to quite another cause. In the second section an attempt has been made to connect this cause with crystalline structure, and to prove its sufficiency to produce the particular phenomena exhibited by crystals. In the third section we find the principle entering into the most complicated instances of these phenomena, and reducing them to cases of extreme simplicity. The choice, therefore, rests between the assumption of *three new forces* which seem but lamely to execute their mission, and that simple modification of existing forces, to which we have given the name elective polarity, and which seems sufficiently embracing to account for all.

It appears then to be sufficiently established, that from the deportment of crystalline bodies in the magnetic field, no direct connection between light and magnetism can be inferred. A rich possession, as regards physical discovery, seems to be thus snatched away from us; but the result will be compensatory. That a certain relation exists, with respect to the path chosen by both forces through transparent bodies, must be evident

* Poggendorff's *Annalen*, vol. lxxvii. p. 578.

to any one who carefully considers the experiments described in this memoir. The further examination of this deeply interesting subject we defer to another occasion.

Nature acts by general laws, to which the terms great and small are unknown; and it cannot be doubted that the modifications of magnetic force, exhibited by bits of copperas and sugar in the magnetic field, display themselves on a large scale in the crust of the earth itself. A lump of stratified grit exhibits elective polarity. It is magnetic, but will set its planes of stratification from pole to pole, though it should be twice as long in the direction at right angles to these planes. A new element appears thus to enter our speculations as to the position of the magnetic poles of our planet; the influence of stratification and plutonic disturbance upon the magnetic and electric forces.

MARBURG: May, 1850.

Note, 1870.—I wish to direct attention here to a paper written by M. Plücker, and translated by myself, for the new series of 'Scientific Memoirs,' published by Taylor and Francis (1853). In this paper M. Plücker approached much more closely than he had previously done to the views expressed in the foregoing memoir. But his paper, which had been written in December, 1849, remained unprinted till 1852.—J. T.

SECOND MEMOIR.

ON DIAMAGNETISM AND MAGNE-CRYSTALLIC ACTION.

[This investigation was conducted in the laboratory of Professor Magnus, of Berlin, during the spring of 1851, and was communicated to the British Association at its meeting at Ipswich the same year. It was also published in the 'Philosophical Magazine' for September, 1851.—J. T. 1870.]

§ 1. *On Diamagnetism.*

Five years ago Faraday established the existence of the force called diamagnetism, and from that time to the present some of the first minds in Germany, France, and England have been devoted to the investigation of this subject. One of the most important aspects of the inquiry is the relation which subsists between magnetism and diamagnetism. Are the laws which govern both forces identical? Will the mathematical expression of the attraction in the one case be converted into the expression of the repulsion in the other by a change of sign from positive to negative?

The conclusions arrived at by Plücker in this field of inquiry are exceedingly remarkable and deserving of attention. His first paper, 'On the relation of Magnetism and Diamagnetism,' is dated from Bonn, September 8, 1847, and will be found in Poggendorff's *Annalen* and in Taylor's 'Scientific Memoirs.' He sets out with the question, 'Is it possible, by mixing a magnetic substance with a diamagnetic, so to balance the opposing forces that an indifferent body will be the result?' This question he answers in the negative. 'The experiments,' he writes, 'which I am about to describe, render it necessary that every thought of the kind should be abandoned.'

One of these experiments will serve as a type of the whole, and will show the foundation on which the negative reply of

M. Plücker rests. A piece of cherry-tree bark, 15 millims. long and 7 millims. wide, was suspended freely between the two movable poles of an electro-magnet; on bringing the points of the poles so near each other that the bark had barely room to swing between them, it set itself, like a diamagnetic substance, with its length *perpendicular* to the line which united the two poles. On removing the poles to a distance, or on raising the bark to a certain height above them, it turned round and set its length *parallel* to the line joining the poles. As is usual, we shall call the former position the *equatorial*, and the latter position the *axial*. Thus when the poles were near, diamagnetism was predominant, and caused the mass to set equatorial; when the poles were distant, magnetism, according to the notion of M. Plücker, was predominant, and caused the mass to set axial. From this he concludes, '*That in the cherry-tree bark two distinct forces are perpetually active; and that one of them, the magnetic, decreases more slowly with the distance than the other, the diamagnetic.*

In a later memoir * this predominance of the diamagnetic force at a short distance is affirmed by M. Plücker to be due to the more general law, that when a magnet operates upon a substance made up of magnetic and diamagnetic constituents, if the power of the magnet be increased, the diamagnetism of the substance increases in a much quicker ratio than the magnetism; so that without altering the distance between it and the magnet, the same substance might at one time be attracted and at another time repelled by merely varying the strength of the exciting current.

This assertion is supported by a number of experiments, in which a watch-glass containing mercury was suspended from one end of a balance. The watch-glass was magnetic, the mercury was diamagnetic. When the glass was suspended at a height of 3·5 millims. above the pole of the magnet, and the latter was excited by a battery of four cells, an attraction of one milligramme was observed; when the magnet was excited by eight cells, the attraction passed over into a repulsion of the same amount.

It is to be regretted that M. Plücker, instead of giving us

* Poggendorff's *Annalen*, vol. lxxv. p. 413.

the actual strength of the exciting current, has mentioned merely the number of cells employed. From this we can get no definite notion as to the amount of magnetic force evolved in the respective cases. It depends of course upon the nature of the circuit whether the current increases with the number of cells or not. If the exterior resistance be small, an advance from four to eight cells will make very little difference; if the outer resistance be a vanishing quantity, one cell is as good as a million.*

During an investigation on the magneto-optic properties of crystals,† which I had the pleasure of conducting in connection with my friend Professor Knoblauch, I had repeated opportunities of observing phenomena exactly similar to those observed by M. Plücker with the cherry-tree bark; but a close study of the subject convinced me that the explanation of these phenomena by no means necessitated the hypothesis of two forces acting in the manner described. Experiment further convinced me, that a more delicate apparatus than the balance used by M. Plücker would be better suited to the measurement of such feeble manifestations of force.

An exact acquaintance with electro-magnetic attractions appeared to be a necessary discipline for the successful investigation of diamagnetic phenomena; and pursuing this idea, an inquiry was commenced last November into the action of an electro-magnet upon masses of soft iron. I was finally led to devote my entire attention to the attraction of soft iron spheres, and the results obtained were so remarkable as to induce me to devote a special memoir to them alone. ‡

In this investigation it was proved, that a ball of soft iron, separated by small fixed distance from the pole of an electro-magnet, was attracted with a force exactly proportional to the square of the exciting current. § Now this attraction is in each case the produce of two factors, one of which represents the magnetism of the magnet, and the other the magnetism of the ball. For example, if the magnetism of the magnet at any

* The usual arrangement of the cells is here assumed; that is, where the negative component of one cell is connected with the positive component of the next.

† Phil. Mag., July 1850.

‡ Phil. Mag., April 1851. Poggendorff's *Annalen*, May 1851.

§ This had been already proved by Lenz and Jacobi, but the employment of the iron spheres renders the result particularly sharp and exact.

given moment be represented by the number 4, and that of the ball by 3, the attraction, which is a consequence of their reciprocal action, is represented by the number 12. If we now suppose the magnetism of the magnet to be doubled by a current of double strength, the ball will have its magnetism also doubled, and the attraction resulting will be expressed by the number 48. Thus we see that a doubling of the power of the magnet causes four times the attraction; and that while the attraction increases as the *square* of the current, *the magnetism of the ball increases in the simple ratio of the current itself.*

Our way to a comparison of magnetism and diamagnetism is thus cleared. We know the law according to which the magnetism of an iron ball increases, and we have simply to ascertain whether the diamagnetism of a bismuth ball follows the same law. For the investigation of this question I constructed the following apparatus.

In two opposite sides of a square wooden box were sawn two circular holes about four inches in diameter. The holes were diagonally opposite to each other, and through each a helix of copper wire was introduced and wedged fast. Each helix contained a core of soft iron, which was pushed so far forward that a line parallel to the sides of the box through which the helices entered, and bisecting the other two sides, was a quarter of an inch distant from the interior end of each core. The distance between the two interior ends was six inches, and in this space a little beam of light wood was suspended. At the ends of the beam two spoon-shaped hollows were worked out, in which a pair of small balls could be conveniently laid. The beam rested in a paper loop, which was attached to one end of a fine silver wire. The wire passed upward through a glass tube nearly three feet in length, and was connected at the top with a torsion head. The tube was made fast in a stout plate of glass, which was laid upon the box like a lid, thus protecting the beam from currents of air. A floor of Bristol board was fixed a little below the level of the axes of the cores, the 'board' being so cut as to fit close to the helices: the two corners of the floor adjacent to the respective cores and diagonally opposite to each other bore each a graduated quadrant. When the instrument was to be used, two balls of the substance to be experimented with were placed upon the spoon-

shaped hollows of the beam and exactly balanced. The balance was established by pushing the beam a little in the required direction through the paper loop in which it loosely rested; and to accomplish this with greater ease, two square pieces were sawn out of the sides of the box, and two others were exactly fitted into the spaces thus opened; these pieces could be taken out at pleasure, and the hand introduced without raising the lid. The torsion-head was arranged so that when the beam bearing the balls came to rest, a thin glass fibre attached to the beam pointed to zero on the graduated quadrant underneath, while the index of the head pointed also to the zero of the graduated circle above. A current was sent through the helices so as to make the two magnetic poles which operated on the diamagnetic balls of opposite names. The balls were repelled when the current flowed. Preserving the current constant, the index above was turned in a direction opposed to the repulsion until the beam stood again at zero. The torsion necessary to effect this is evidently the expression of the repulsive force exerted at this particular distance.

Fig. 1 represents the appearance of the beam and helices when looked down upon through the glass lid. Fig. 2 represents the beam and balls attached to the suspending wire.

Fig. 1

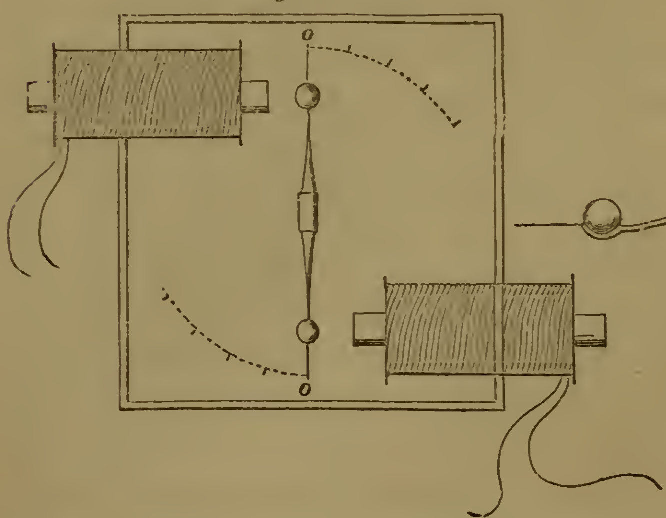
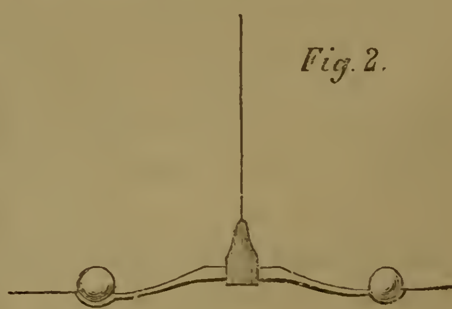


Fig. 2



When the glass index pointed to zero, an interval of about $\frac{1}{12}$th of an inch usually separated the nearest surfaces of the diamagnetic balls from the core ends. The intensity of the current

was measured by a tangent galvanometer, and it was varied by means of a rheostat. Always before commencing a series of experiments, the little beam was proved. With very strong currents it was found to be slightly diamagnetic; but so feeble, that its action, even supposing it not to follow the same law of increase as the ball (which, however, it certainly does), could cause no measurable disturbance.

I neglected no precaution to secure the perfect purity of the substances examined. The entire investigation was conducted in the private cabinet of Professor Magnus in Berlin; and at the same time Dr. Schneider happened to be engaged in the professor's laboratory in determining the chemical equivalent of bismuth. He was kind enough to give me a portion of this substance, prepared in the following way:—The metal of commerce was dissolved in nitric acid and precipitated with distilled water; whatever iron was present remained in the solution. The precipitate was filtered, washed for six days successively, and afterwards reduced by means of black flux. The metal thus obtained was again melted in a Hessian crucible, and saltpetre was gradually added, the mass at the same time being briskly stirred. Every remaining trace of foreign ingredient was thus oxidised and rose to the surface, from which it was carefully skimmed. The metal thus purified was cast into a bullet-mould, the interior surface of which was coated by a thin layer of oil; the outer surface of each bullet was carefully scraped away with glass, the ball was then scoured with sea-sand, and finally boiled in hydrochloric acid. The bismuth balls thus purified were placed upon the hollows of the beam, Fig. 2, and their repulsions by currents of various strengths determined in the manner indicated. The series of repulsions thus obtained are exactly analogous to the series of attractions in the experiments with the balls of iron. Now the square roots of the attractions give a series of numbers exactly proportional to the currents employed; and the question to be decided is,—'Will the square roots of the repulsions give a similar series, or will they not?'

Calling the angle which the needle of the tangent compass, under the influence of the current, makes with the magnetic meridian a, then if the repulsion of the bismuth ball follow the same law as the attraction of the iron one, we shall have the equation

$$\sqrt{T} = n \tan a,$$

where T represents the torsion necessary to bring the beam back to zero, and n is a constant depending on the nature of the experiment. The following tables will show the fulfilment or non-fulfilment of this equation:—

Table I.—Bismuth spheres, 8 millims. diameter.
$n = 11{\cdot}7$.

a	tan a	T	$\sqrt{T}$	n tan a
10°	0·176	5	2·23	2·06
20	0·364	16·3	4·04	4·25
30	0·577	42·3	6·50	6·74
35	0·700	64	8	8·19
40	0·839	100	10	9·81
45	1·000	136	11·66	11·7
50	1·192	195	13·96	13·95

A second series was made with a pair of spheres of the bismuth of commerce with the same result.

Sulphur is also a diamagnetic substance, but a much weaker one than bismuth. The next series of experiments were made with two balls of this substance.

Table II.—Sulphur spheres, 8 millims. diameter.
$n = 3{\cdot}3$.

a	tan a	T	$\sqrt{T}$	n tan a
20° 0′	0·364	1·2	1·10	1·20
30 45	0·595	3·0	1·73	1·96
41 20	0·880	8·0	2·83	2·90
54 0	1·376	21·0	4·58	4·54

A pair of sulphur balls were next taken of nearly twice the diameter of the preceding.

Table III.—Sulphur spheres, 13·4 millims. diameter.
$n = 6{\cdot}7$.

a	tan a	T	$\sqrt{T}$	n tan a
20° 0′	0·364	6·2	2·45	2·44
30 45	0·595	15·0	3·87	3·98
41 20	0·880	34·5	5·90	5·89
54 0	1·376	89·0	9·43	9·22

The sulphur from which these balls were made was the material of commerce. After the experiments one of the balls was placed in a clean porcelain crucible and brought over the flame of a spirit-lamp; the sulphur melted, ignited, and disappeared in sulphurous acid vapour. A portion of solid substance remained in the crucible unvolatilised. This was dissolved in hydrochloric acid, and ferrocyanide of potassium was added; the solution turned immediately blue; iron was present. The other ball was submitted to a similar examination, and with the same result; both balls contained a slight admixture of iron.

In this case, therefore, the two opposing forces, magnetism and diamagnetism, were actually present, but we find the equation $\sqrt{T}=n \tan \alpha$ fulfilled notwithstanding. Did one of the forces increase with the ascending magnetic power more quickly than the other, this result would be impossible.

Flowers of sulphur were next tried, but found to contain a considerable quantity of iron. I have to thank Professor Magnus for a portion of a native crystal of the substance obtained in Sicily, which upon trial was found to be perfectly pure. From this two small pellets were formed and laid upon the torsion-balance: they gave the following results:—

Table IV.—Spheres of Native Sulphur.
$n=2{\cdot}65$.

α	$\tan \alpha$	T	$\sqrt{T}$	$n \tan \alpha$
20°	0·364	0·9	0·95	0·96
30	0·577	2·5	1·58	1·53
40	0·839	5·0	2·24	2·22
45	1·000	7·0	2·64	2·65
50	1·192	10·0	3·16	3·16

The next substance chosen was calcareous spar. The corners of the crystalline rhomb were first filed away, and the mass thus rendered tolerably round; it was then placed between two pieces of soft sandstone, in each of which a hollow, like the cavity of a bullet-mould, had been worked out. By turning the stones, one right and the other left, and adding a little water, and a little patience, the crystal was at length reduced to a spherical form. The ball was then washed, and its surface care-

fully cleansed in dilute hydrochloric acid. The first pair of balls were from the neighbourhood of Clitheroe in Lancashire.

Table V.—Spheres of Calcareous Spar, 9·2 millims. diameter. $n=3·7$.

α	tan α	T	$\sqrt{T}$	n tan α
20°	0·364	1·8	1·34	1·34
25	0·466	3·0	1·73	1·72
30	0·577	4·5	2·12	2·13
35	0·700	7·0	2·64	2·59
40	0·839	9·7	3·11	3·10
45	1·000	14·0	3·74	3·70

The spar from which these balls were taken was not quite transparent; to ascertain whether its dullness was due to the presence of iron, a crystal which weighed about 3 grammes was dissolved in hydrochloric acid; the solution was exposed in a flat basin to the air, and the iron, if present, suffered to oxidise; ferrocyanide of potassium was added, but not the slightest tinge indicative of iron was perceptible.

Experiments were next made with a pair of spheres of calcareous spar from Andreasberg in the Harz Mountains.

Table VI.—Spheres of Calcareous Spar, 10·8 millims. diameter. $n=5$.

α	tan α	T	$\sqrt{T}$	n tan α
20° 0′	0·364	2·8	1·68	1·82
25 0	0·466	5·0	2·21	2·33
30 0	0·577	8·0	2·83	2·83
35 0	0·700	11·2	3·35	3·50
37 30	0·767	14·5	3·81	3·83
57 0	1·540	60·0	7·75	7·70

The spar from which these balls were taken was perfectly transparent. After the experiment, they were partially dissolved in hydrochloric acid, and the solution tested as in the former case for iron. No trace of iron was present.

The conclusion to be drawn from all these experiments, and from many others which I forbear citing, is, that the law of increase for a diamagnetic body is exactly the same as for a

magnetic—a result irreconcilable with that arrived at by M. Plücker. I had proceeded further with this investigation than the point now attained, when I learned that a memoir on diamagnetism by M. Edmond Becquerel had appeared in the May number of the *Annales de Chimie et de Physique.** In this memoir the views of Plücker are also controverted, and a number of experiments are adduced to prove the identity of the laws which regulate magnetic attraction and diamagnetic repulsion. The argument employed by M. Becquerel is the same in principle as that furnished by the foregoing experiments. He proves that the repulsion of *bars* of bismuth, sulphur and wax, increases as the square of the exciting current, and that the attraction of a little bar of iron follows the same law. We have both been guided in our inquiries by the same fundamental thought, though our modes of carrying out the thought are different.

I have observed many phenomena, which, without due consideration, would lead us directly to Plücker's conclusions; and a few of which may be here described. The bismuth balls were placed upon the beam, and one core was excited; on the top of the ball opposite, a particle of iron, not the twentieth part of a common pin-head in size, was fixed. A current of 10° circulated in the helix, and the beam came to rest at the distance of 4° from the zero of the under graduation. The current was then permitted to increase gradually. The magnetism of the iron particle and the diamagnetism of the bismuth rose of course along with it, but the latter triumphed; the beam was repelled, and finally came to rest against a stop which was placed 9° distant.

* In fact M. Edmond Becquerel had proved, in the year 1850, that diamagnetic repulsion followed the law of squares. My experiments on this subject, though different in form, are to be regarded as mere verifications of his. See *Annales de Chimie et de Physique*, vol. xxviii. p. 301. In the very able memoir referred to in the text, he amply illustrates the law of attraction and repulsion; and there also he repeats the theoretic conclusion already adverted to, which in his own words is this:—

'Cette hypothèse consiste à supposer qu'il n'y a pas deux genres d'actions différentes produites sur les corps par les aimants, actions magnétiques et actions diamagnétiques, mais bien un seul genre d'action, une aimantation par influence, et que la répulsion exercée sur les substances qui s'éloignent des pôles des aimants est due à ce que les corps sont entourés par un milieu plus magnétique qu'elles.'

'Je n'ai présenté,' he adds, 'cette explication du diamagnétisme que pour lier entre eux, d'une manière plus simple, je crois, qu'on ne l'avait fait jusqu'ici, les effets du diamagnétisme sur les différents corps soumis à son action.'—*Annales de Chimie et de Physique*, vol. xxxii. p. 112.

The particle of iron was removed, and a small crystal of carbonate of iron was put in its place; a current of 15° circulated in the helix, and the beam came to rest at about 3° distant from zero. The current was raised gradually, but before it had reached 30°,* diamagnetism conquered, and the beam receded to the stop as before.

Thinking that this apparent triumph of diamagnetism might be due to the fact that the crystal of carbonate of iron had become saturated with magnetism, and that it no longer followed the law of increase true for a larger piece of the substance, I tested the crystal with currents up to 49°; the attractions were exactly proportional to the squares of the exciting currents.

Thinking also that a certain reciprocal action between the bismuth and the crystal, when both were placed together in the magnetic field, might so modify the latter as to produce the observed result, I removed the crystal, and placed a cube of the zinc of commerce upon the opposite end of the beam. The zinc was slightly magnetic. Bismuth and zinc were thus separated by an interval of 6 inches; both cores were excited by a current of 10°, and the beam, after some oscillations, came to rest at 4° distant from zero. The current was now gradually raised, but when it reached 35° of the graduated quadrant, the beam receded and was held firmly against the stop. When the circuit was broken it left the stop, and, after some oscillations, came to rest at zero.

These experiments seem fully to bear out the notion of Plücker. In each case we waited till both forces were in equilibrium; and it might be thought that if the forces followed the same law, the beam ought not to move. Let us, however, clear the experiment of all mystery. When the beam was in equilibrium with a current of 10°, let us ask what forces were opposed to the repulsion of the bismuth? There was, first of all, the attraction of the zinc; but besides this, there was a torsion of 4°; for the position of equilibrium for the beam with the unexcited magnet was at zero. Let us suppose the magnetism of the zinc at the distance of 4, and with the current 10°, to be equal to 8 of torsion; this, added to the 4 already present, will give the force opposed to the bismuth; the repulsion of the

* Currents of 10°, of 15°, of 30°, &c. signify currents which produced these respective deflections of the tangent-compass needle.

latter is therefore equal to 12. Let us now conceive the current raised from 10° to 35°, that is quadrupled.* Supposing the magnetism of the zinc to be increased in proportion to the strength of the current, its attraction will now be 32; this, added to 4 of torsion, which remains constant, makes 36, which is therefore the force exerted against the bismuth by a current of 35° under the present circumstances. But the repulsion of the bismuth being also quadrupled, it is now 48. This, opposed to a force of 36, necessarily conquers, and the beam is repelled.

We thus see that, although the magnetic force on one side, and the diamagnetic on the other side, follow precisely the same law, the introduction of the small constant 4° entirely destroys the balance of action, so that to all appearance diamagnetism increases in a much quicker ratio than magnetism. Such a constant has probably crept into the experiments of Plücker; an inadvertency not to be wondered at, when we remember that the force was new at the time, and our knowledge of the precautions necessary to its accurate investigation very imperfect.

§ 2. *On Magne-crystallic Action.*

Plücker has discovered that, when a crystal of pure carbonate of lime is suspended in the magnetic field with its optic axis horizontal, the said axis always sets itself equatorial. He attributed this action of the spar to a repulsion of the optic axis by the magnet, which is independent of the magnetism or diamagnetism of the mass of the crystal. It was the product of a new force, which Mr. Faraday has named 'the optic axis force.'

In the memoirs published by Knoblauch and myself, this view is dissented from, and it is there proved that the action of the crystal, so far from being independent of the magnetism or diamagnetism of its mass, is totally changed by the substitution of a magnetic constituent for a diamagnetic. Our experiments led us to the conclusion, that the position of the crystal of carbonate of lime was due to the superior repulsion of the mass of the crystal in the direction of the optic axis. This view, though

* The tangent of 35° being four times the tangent of 10°.

supported by the strongest presumptive facts, has remained up to the present time without direct proof; if, however, a difference of repulsion, such as that we have supposed, actually exists, it may be expected to manifest itself upon the torsion-balance.

But the entire repulsion of calcareous spar is so feeble, that to discover a differential action of this kind requires great nicety of experiment. I returned to this subject three different times; twice I failed, and despaired of being able to establish a difference with the apparatus at my command. But the thought clung to me, and after an interval of some weeks, I resolved to try again.*

The spheres of calcareous spar were placed upon the beam, and the latter was exactly balanced. The index above was so placed, that when the beam came to rest, the attached glass fibre exactly coincided with a fine black line drawn upon the Bristol board underneath. Two dots were placed upon the glass cover, about the fiftieth of an inch asunder, and the fibre was observed through the interval between them. The beam was about four inches below the cover, and parallax was thus avoided. On exciting both cores the balls receded, the index of the torsion-head was softly turned against the recession, till the fibre was brought once more into exact coincidence with the fine black line, and the torsion necessary to effect this was read off upon the graduated circle above.

The repulsion of the spheres was measured in four different directions:—

1. The optic axes were parallel to the axes of the iron cores.

2. The spheres were turned through an arc of 90°, so that the optic axes were at right angles to the cores.

3. The spheres were turned 90° in the same direction, so that the other ends of the axes faced the cores.

* 'The torsion balance was placed before a window through which the sun shone in the forenoon. In experimenting with spheres of bismuth, I was often perplexed and baffled by the contradictory results obtained at different hours of the same day. With spheres of calcareous spar, where the diamagnetic action was weaker, the discrepancies were still more striking. Once while gazing puzzled at the clear ball of spar resting on the torsion balance, my attention was drawn to the bright spot of sunlight formed by the convergence of the rays which traversed the spar, and the thought immediately occurred to me that this little "fire-place" might create currents of air strong enough to produce the observed anomalies. The shutting out of the light entirely removed the cause of disturbance; which however was mainly due to the heating of the glass lid of the balance.'—Phil. Mag. vol. iii. p. 128.

4. The spheres were turned 90° further, so that their axes were again at right angles to the cores, but with the opposite surface to that in (2) facing the latter.

The following are the respective repulsions :—

	Repulsion
1st position	28·5
2nd position	26·5
3rd position	27·0
4th position	24·5
[Mean of repulsions along optic axis	27·8
,, ,, across ,, ...	25·5
Or as 100 : 91·7]	

Each of the helices surrounding the cores was composed of two insulated wires; the four ends of these could be so combined that the current could pass through both at the same time, as if they were a single wire, or it could be caused to traverse one wire after the other. The first arrangement was advantageous when a small exterior resistance was an object to be secured, the second when the force of the battery was such as to render exterior resistance to a certain extent a matter of indifference. In the foregoing experiments the first of these arrangements was adopted. Before commencing, I had taken fresh acid and freshly amalgamated zinc cylinders, so that the battery was in good condition. The second arrangement was then adopted, that is to say, the current was allowed to traverse one wire after the other, and the following repulsions were observed; the numbers refer to the positions already indicated :—

	Repulsion
1st position	57
2nd position	51
3rd position	53
4th position	48
[Mean of repulsions along optic axis	55
,, ,, across ,, ...	49·5
Or as 100 : 90]	

These experiments furnish the direct proof that calcareous spar is repelled most strongly in the direction of the optic axis. That Mr. Faraday has not succeeded in establishing a difference here is explained by reference to his mode of experiment. He observed the distance to which the spar was repelled, and found

this the same for all positions of the crystal. The magnetic force at this distance is too weak to show a difference. In the above experiments, on the contrary, the crystal was forced back into a portion of the magnetic field where the excitement was intense, and here for the first time the difference rises to a measurable quantity.

Carbonate of iron is a crystal of the same form as calcareous spar, the iron filling up, so to speak, the exact space vacated by the calcium. This crystal is strongly magnetic; suspended in the magnetic field, that line which in calcareous spar sets equatorial, sets here axial, but with an energy far surpassing the spar; a greater differential action may therefore be anticipated.

A pair of spheres were formed from the crystal, but their attraction was so strong, that to separate them from the magnet would strain the wire beyond its limits of elasticity; one sphere only could therefore be used, the other being used as a balance-weight merely. The core opposite to the latter was removed, and the current sent round that helix only which surrounded the former. A piece of Bristol board was placed against the end of the core, and the torsion-head was so turned that when the index above pointed to zero, the little sphere was on the verge of contact. The magnet was then excited and the sphere attracted. The index was then turned in a direction opposed to the attraction until the ball gave way; the torsion necessary to effect this expresses the attraction. The crystal was first placed so that its axis was parallel to that of the magnet, and afterwards so that it was perpendicular to the same. The following tables exhibit the results in both cases respectively:—

Table VII.—Carbonate of Iron. Axis of Crystal parallel to axis of Magnet. $n=25\cdot5$.

a	tan a	T	$\sqrt{T}$	n tan a
15	0·268	43	6·56	6·57
20	0·364	80	8·94	8·91
25	0·466	129	11·36	11·42
30	0·577	200	14·14	14·14

Table VIII.—Carbonate of Iron. Axis of Crystal perpendicular to axis of Magnet. $n = 20 \cdot 7$.

a	$\tan a$	T	$\sqrt{T}$	$n \tan a$
15	0·268	30·5	5·52	5·55
20	0·364	56·0	7·48	7·53
25	0·466	92·5	9·62	9·64
30	0·577	142·5	11·44	11·44

We learn from these experiments that the law according to which the attraction of carbonate of iron increases, is exactly the same as that according to which the repulsion of the calcareous spar increases, and that the respective forces manifest themselves in both cases with the greatest energy in the direction of the optic axis, the attraction along the optic axis being to that across the same axis, in all four cases, as 100 : 71 nearly.

Let us observe for an instant the perfect antithesis which exists between carbonate of lime and carbonate of iron. The former is a diamagnetic crystal. Suspended before the single pole of a magnet, the entire mass is repelled, but the mass in one direction is repelled with peculiar force, and this direction, when the crystal is suspended in the magnetic field, recedes as far as possible from the poles, and finally sets equatorial. The crystal of carbonate of iron is, on the contrary, strongly magnetic; suspended before a single pole the entire mass is attracted, but in one direction the mass is attracted with peculiar energy, and this direction, when the crystal is suspended in the magnetic field, will approach the poles and finally set axial.

Sulphate of iron in the magnetic field displays a directive action considerably inferior to that of carbonate of iron. Some large crystals were obtained from a chemical manufactory, and from these I cut two clean cubes. Each was suspended by a cocoon fibre in the magnetic field, and the line which stood axial was marked upon it. The white powder which collects by efflorescence around these crystals was washed away, and two transparent cubes remained. These were laid upon the torsion-balance, and instead of the Bristol board used in the last experiment, two plates of glass were placed against the core ends; the adhesion of the cubes, which in delicate experiments

of this nature sometimes enters as a disturbing element, was thus reduced to a minimum. As in the case of carbonate of iron, one core only was excited. The cube opposite to this core was first so placed that the line which stood axial in the magnetic field was parallel to the axis of the core; preserving this line horizontal, the three remaining faces were presented successively to the core, and the attraction measured in each particular case; these attractions were as follows:—

Cube of sulphate of iron, edges 10 *millims.*

	Attraction
1st position	43·0
2nd position	36·3
3rd position	40·0
4th position	34·5
[Mean of attraction along axis	41·5
„ „ across „	35·4
Or as 100 : 85 nearly]	

From an article translated from Poggendorff's *Annalen*, and published in the June number of the 'Philosophical Magazine,' it will be seen that Prof. Plücker has experimented with a cube of sulphate of iron, and has arrived at results which he adduces against the theory of magne-crystallic action advanced by Knoblauch and myself. He rightly concluded that if the position of the crystal, suspended between two poles, were due to the superior attraction exerted in a certain direction, this peculiarity ought to exhibit itself in the attraction of the entire mass of the crystal by the single pole of a magnet. He brings this conclusion to the test of experiment, suspends the crystal from one end of a balance, weighs the attraction in different directions, but finds no such difference as that implied by the conclusion. This result, I believe, is entirely due to the imperfection of his apparatus; I have tried a very fine balance with even worse success than M. Plücker. Although the torsion-balance furnishes a means of experiment immeasurably finer, still, with it, great delicacy of manipulation and a considerable exercise of patience are necessary to insure invariable success.

Faraday has discovered, that if a bismuth crystal be suspended in the magnetic field, it will set itself so that a line perpendicular to the plane of most eminent cleavage will be axial; this line he calls the magne-crystallic axis of the crystal. In the memoir by Knoblauch and myself before alluded to, the

position of the magne-crystallic axis is affirmed to be a secondary result, depending on the fact that the mass in the direction of the planes of cleavage is most strongly repelled. The general fact of superior repulsion in the direction of the cleavages has been already demonstrated by Mr. Faraday.

Our torsion-balance furnishes us with a quantitative confirmation of Mr. Faraday's result. Two cubes of bismuth were prepared, in each of which the plane of most eminent cleavage formed two of the opposite sides. Suspended by a fibre of cocoon-silk in the magnetic field, the line perpendicular to the cleavage turned into the axial position, or what amounts to the same as far as the eye is concerned, the cleavage itself receded from the poles and stood equatorial. These cubes were placed one on each end of the torsion-balance; first, so that the plane of most eminent cleavage was parallel to the axes of the cores, and afterwards perpendicular to these axes. The respective repulsions are stated in the following tables.

Table IX.—Cubes of bismuth, edges 6 millims. Plane of most eminent cleavage parallel to axes of cores.

a	T
20	11·7
30	34·8
40	78
45	111
50	153

Table X.—The same cubes. Plane of most eminent cleavage perpendicular to axes of cores.

a	T
20	8
30	23
40	53
45	76·5
50	110

A comparison of these two tables shows us that the repulsion

of the cubes, when the plane of most eminent cleavage was parallel to the magnetic axis, is to the repulsion when the said plane was perpendicular thereto in the ratio nearly of 100 : 71.

What is it, then, which causes this superior manifestation of force in a certain direction? To this question experiment returns the following reply :—‘ If the arrangement of the component particles of any body be such as to present different degrees of proximity in different directions, then the line of closest proximity, other circumstances being equal, will be that of strongest attraction in magnetic bodies and of strongest repulsion in diamagnetic bodies.’

The torsion-balance enables us to test this theory. A quantity of bismuth was ground to dust in an agate mortar, gum-water was added, and the mass was kneaded to a stiff paste. This was placed between two glasses and pressed together; from the mass when dried two cubes were taken, the line of compression being perpendicular to two of the faces of each cube and parallel to the other four. Suspended by a silk fibre in the magnetic field, upon closing the circuit the line of compression turned strongly into the equatorial position, exactly as the plane of most eminent cleavage in the case of the crystal. The cubes were placed one upon each end of the torsion-balance; first with the line of compression parallel to the cores, and secondly with the same line perpendicular to the cores. The following are the repulsions exhibited in both cases respectively.

Table XI.—Cubes of powdered bismuth, edges 7 millims. Line of compression parallel to axes of cores.

α	tan α	T	$\sqrt{T}$	$8{\cdot}3 \times \tan\alpha$
30	0·577	22	4·69	4·78
40	0·839	46	6·78	6·96
45	1·000	67	8·19	8·30
50	1·192	98	9·89	9·89

From this table we see that the law of increase for the artificial cube is the same as that for diamagnetic substances generally.

Table XII.—The same cubes. Line of compression perpendicular to cores.

a	T
30	13
40	31
45	46
50	67

A comparison of both tables shows us that the line which stands equatorial in the magnetic field is most strongly repelled upon the torsion-balance, exactly as in the case of the crystal; the repulsion in the direction of this line and in a direction perpendicular to the same being nearly in the ratio of 100 : 66 nearly. Similar experiments were made with cubes of powdered carbonate of iron. The line of compression set axial in the magnetic field, and on the torsion-balance the attraction along this line was a maximum.

[*Summary.*—Differential attractions and repulsions of magnetic and diamagnetic bodies:—

	Along axis	Across axis
Carbonate of iron (attraction)	100	71
Carbonate of lime (repulsion)	100	90
Sulphate of iron (attraction)	100	85
Bismuth (repulsion)	100	71
	Along line of pressure	Across line of pressure
Compressed bismuth	100	66

In all cases in magnetic bodies the line of strongest attraction sets from pole to pole, while in diamagnetic bodies the line of strongest repulsion sets equatorial.]

At the last meeting of the British Association, an objection, which will probably suggest itself to all who study the subject as profoundly as he has done, was urged, *viva voce*, against this mode of experiment by Professor William Thomson. 'You have,' he said, 'reduced the mass to powder, but you have not thereby destroyed the crystalline property; your powder is a collection of smaller crystals, and the pressing of the mass together gives rise to a predominance of axes in a certain direction; so that the repulsion and attraction of the line of compression which you refer to the mere closeness of aggregation is, after all, a product of crystalline action.'

I know that this objection, which was specially directed against the experiment made with powdered bismuth and carbonate of lime, floats in the minds of many both in Germany and England, and I am therefore anxious to give it a full and fair reply. I might urge, that in the case of the bismuth powder at least, the tendency of compression would be to place the little component crystals in such a position, that a deportment precisely the reverse of that actually observed might be anticipated. If we pound the crystal to the finest dust, the particles of this dust, to render Mr. Thomson's hypothesis intelligible, must have a certain predominant shape, otherwise there is no reason to suppose that pressure will *always* cause the axes of the little crystals to take up the same predominant direction. Now what shape is most likely here? The crystal cleaves in one direction more easily than in any other; is it not then probable that the powder will be chiefly composed of minute scales, whose opposite flat surfaces are the surfaces of principal cleavage? And what is the most probable effect of compression? Will it not be to place these little scales with their flat surfaces perpendicular to the line in which the pressure is exerted? In the crystal, the line perpendicular to the principal cleavage sets axial, and hence it might be expected that the line of compression in the model would set axial also; it does not, however,—it sets equatorial.

This, however, though a strong presumptive argument, is not yet convincing; and it is no easy matter to find one that shall be so. Bismuth powder will remain crystalline, and carbonate of lime is never free from suspicion. I thought I had found an unexceptionable substance in chalk, inasmuch as Ehrenberg has proved it to be a mere collection of microscopic shells; but Professor Ehrenberg himself informs me, that even these shells, which require a high magnifying power to render them visible, are in their turn composed of infinitesimal crystals of calcareous spar. In this dilemma one way remains open to us: we will allow the objection to stand, and follow it out to its inevitable consequences; if these are opposed to fact, the objection necessarily falls.

Let us suppose the bismuth powder to be rearranged, so that the perfect crystal from which it was obtained is restored. In this case the axes of all the little component crystals are

parallel, they work all together, and hence their action must be greater than if only a majority of them were parallel. In a bismuth *crystal*, therefore, the difference of action in the line of the magne-crystallic axis, and in a line perpendicular thereto, must be a maximum. It must, for example, be greater than any difference which the model of bismuth powder can exhibit; for a portion of the force attributed to the axes must in this case be annulled by the confused grouping of the little component crystals. In the words of Professor Thomson, it is merely a balance of action brought about by predominance, which can make itself manifest here. Hence, if we measure the repulsion of the crystal in a direction parallel to the principal cleavage, and in a direction perpendicular to it, and also measure the repulsion of the model in the line of compression and in a line perpendicular to it, the ratio of the two former repulsions, that is, of the first to the second, must be greater than the ratio of the two latter, that is, of the third to the fourth.

Turning to Tables IX. and X., we see that the ratio of the repulsion of the crystal in the direction of principal cleavage to the repulsion in a direction perpendicular to the same is expressed by the fraction $\frac{15}{11} = 1{\cdot}36$. Turning to Tables XI. and XII., we find that the ratio of the repulsion of the model in the line of compression to the repulsion in a line perpendicular to it is expressed by the fraction $\frac{3}{2} = 1{\cdot}5$. In the latter case, therefore, we have the greatest differential effect; which result, were the repulsion due to the mere predominance of axes, as urged by Mr. Thomson, would be tantamount to the conclusion that a part is greater than the whole. This result has been entirely unsought. The models were constructed with the view of establishing the general fact, that the repulsion in the line of compression is greatest. That this has fallen out in the manner described is a pure accident. I have no doubt whatever that models might be made in which this difference of action would be double of that exhibited by the crystal.

The case, however, is not yet free from suspicion; the gum-water with which it is necessary to bind the powder may possibly exert some secret influence. When isinglass or jelly is compressed, we know that it exhibits optical phenomena similar

to those exhibited by crystals; and the squeezing of the metallic dough may induce a kind of crystalline structure on the part of the gum sufficient to produce the phenomena observed.

An experiment to which I was conducted by the following accident will set this doubt, and I believe all other doubts regarding the influence of compression, completely at rest. Having repeated occasion to refer to the deportment of crystals in the magnetic field, so as to be able to compare this deportment with the attraction or repulsion of the entire mass upon the torsion-balance, through the kindness of Professor Magnus, the great electro-magnet of the University of Berlin* was placed in the room where I experimented. One morning a cube of bismuth was suspended between the movable poles, and not knowing the peculiarities of the instrument, I chanced to bring the poles too near each other. On closing the circuit, the principal cleavage of the crystal receded to the equator. Scarcely however was this attained, when the poles were observed moving towards each other, and before I had time to break the circuit, they had rushed together and caught the crystal between them. The pressure exerted squeezed the cube to about three-fourths of its former thickness, and it immediately occurred to me that the theory of proximity, if it were true, ought to tell here. The pressure brought the particles of the crystal in the line of compression more closely together, and hence a modification, if not an entire subversion of the previous action, was to be expected.

Having liberated the crystal, I boiled it in hydrochloric acid, so as to remove any impurity it might have contracted by contact with the iron. It was again suspended between the poles, and completely verified the foregoing anticipation. The line of compression, that is, the magne-crystallic axis of the crystal. which formerly set from pole to pole, now set strongly equatorial. I then brought the poles intentionally near each other, and allowed them to close once more upon the already compressed cube; its original deportment was thereby restored. This I repeated several times with several different crystals, and with the same unvarying result; the line of compression always stood equatorial, and it was a matter of perfect indifference whether this line was the magne-crystallic axis or not. The ex-

* A notion of the power of this instrument may be derived from the fact, that the copper helices alone which surrounded the pillars of soft iron weighed 243 pounds.

periment was then repeated with a common vice. I rubbed the letters from two copper coins with sandstone, and polished the surfaces; between the plates thus obtained various pieces of bismuth were forcibly squeezed; in this way plates were procured about as thick as a shilling, and from half an inch to an inch in length. Although the diamagnetism of the substance tended strongly to cause such a plate, suspended from its edge between the poles, to take up the equatorial position, although the force attributed to the magne-crystallic axis worked in each case in unison with the diamagnetism of the mass, every plate set nevertheless with its length from pole to pole, and its magne-crystallic axis equatorial.

This superior repulsion of the line of compression manifests itself upon the torsion-balance also. The cubes of bismuth crystal already made use of were squeezed in a vice to about four-fifths of their former thickness; the line of compression in each case being perpendicular to the principal cleavage, and consequently parallel to the magne-crystallic axis. From the masses thus deformed, two new cubes were taken; these laid upon the torsion-balance in the positions indicated in the tables, gave the following results:—

Table XIII.—Bismuth crystals, compressed cubes. Plane of most eminent cleavage parallel to axes of magnets.

a	T
20	7·8
30	21
40	47
45	67
50	101

Table XIV.—The same cubes. Plane of most eminent cleavage perpendicular to axes of magnets.

a	T
20	9
30	25·5
40	57·3
45	79
50	113

Looking back to Tables IX. and X., we see that the line which was there repelled most strongly is here repelled most feebly, and *vice versâ*, the change being due to compression. The ratio there is 100 : 71; here it is 100 : 112 nearly.

I have been careful to make similar experiments with substances concerning whose amorphism there can be but little doubt. A very convenient substance for showing the influence of compression is the white wax used in candles. The substance is diamagnetic. A little cylinder of the wax suspended in the magnetic field set with its axis equatorial. It was then placed between two stout pieces of glass and squeezed as thin as a sixpence; suspended from its edge, the plate thus formed set its length, which coincided with the axis of the previous cylinder, axial, and its shortest dimension equatorial.

The plate was then cut into little squares, which were laid one upon the other and then pressed together to a compact cubical mass. Two such cubes were placed upon the torsion-balance, and the repulsion in the line of compression, and in a line perpendicular to the same, were determined—the former was considerably the greater.

The pith, scooped from a fresh roll, was placed between the glass plates, and squeezed closely together; after remaining in the vice for half an hour, a rectangle was taken from the plate thus formed, and suspended from its edge in the magnetic field; it set like a magnetic body, with its length from pole to pole. The mass was diamagnetic, its line of compression was repelled, and an apparent attraction of the plate was the consequence.

Fine wheat-flour was mixed with distilled water into a stiff paste, and the diamagnetic mass was squeezed into thin cakes. The cakes when suspended from the edges set always with their longest dimension from pole to pole, the line of compression being equatorial.

Rye-flour, from which the Germans make their black bread, was treated in the same manner and with the same result.

I have an oblong plate of shale from the neighbourhood of Blackburn in Lancashire, which imitates M. Plücker's first experiment with tourmaline with perfect exactitude. The mass is magnetic, like the tourmaline. Suspended from the centre of one of its edges, it sets *axial*; this corresponds to the position of the tourmaline when the optic axis is vertical. Suspended

from the centre of the adjacent edge, it sets even more strongly *equatorial*; this corresponds with the tourmaline when the optic axis is horizontal. If the eyes be closed, and the respective positions of the plate of shale ascertained by means of touch, and if the same be done with Plücker's plate of tourmaline, it will be impossible to distinguish the one deportment from the other.

With regard to the experiment with the cherry-tree bark, I have a bar of chemically pure bismuth which does not contain a trace of magnetism, and which exhibits the precise phenomena observed with the bark. These phenomena do not therefore necessitate the hypothesis of two conflicting forces, the one or the other of which predominates according as the poles of the magnet are more or less distant. I have already commenced an investigation in which the deportment of the bark and other phenomena of an analogous nature will be more fully discussed.

Every inquirer who has occupied himself experimentally with electro-magnetic attractions must have been struck with the great and speedy diminution of the force by which soft iron is attracted, when the distance is augmented, in the immediate neighbourhood of the poles. In experiments with spheres of soft iron, I have usually found that a distance of $\frac{1}{100}$th of an inch between the sphere and the magnet is sufficient to reduce the force with which the former is attracted to $\frac{1}{10}$th of the attraction exerted when the sphere is in contact. To any one acquainted with this fact, and aware, at the same time, of the comparative sluggishness with which a bismuth ball moves in obedience to the repulsive force even when close to the poles, a law the exact reverse of that affirmed by M. Plücker must appear exceedingly probable.

The bismuth balls were placed upon the torsion balance; on the top of one of them a particle of iron filing was fixed, and with this compound mass the space opposite to a core excited by a current of 50° was sounded. The beam was brought by gentle pushing into various positions, sometimes close to the magnet, sometimes distant. The position of equilibrium for the beam when the core was unexcited was always zero. When the beam was pushed to a distance of 4° (about $\frac{3}{10}$ths of an inch) from the core end, on exciting the magnet it receded still further and rested against a stop at 9° distant. When the current

was interrupted the beam left the stop and approached the core; but if, before it had attained the third or fourth degree, the circuit was closed, the beam was driven back and rested against the stop as before.

Preserving the current constant at 50°, the index of the torsion-head was turned gently against the repulsion, and in this way the ball was caused slowly to approach the magnet. The repulsion continued until the glass fibre of the beam pointed to 2°; here an *attractive* force suddenly manifested itself, the ball passed speedily on to contact with the core end, to separate it from which a torsion of 50° was requisite.

The circuit was broken and the beam allowed to come to rest at zero, a space of about $\frac{1}{12}$th of an inch intervening between the ball and the end of the magnet; on closing the circuit the beam was *attracted*. The current was once more interrupted, and the torsion-head so arranged, that the beam came to rest at 3° distant; on establishing the current again the beam was *repelled*. Between 0° and 3° there was a position of unstable equilibrium for the beam; from this place to the end of the magnet attraction was triumphant, beyond this place repulsion prevailed.

Here we see, that on approaching the pole, the attraction of the magnetic particle mounts much more speedily than the repulsion of the diamagnetic ball; a result the reverse of that arrived at by M. Plücker, but most certainly coincident with that which everybody who has studied electro-magnetic attractions would expect. Shall we therefore conclude that 'magnetism' increases more quickly than 'diamagnetism?' The experiment by no means justifies so wide a generalisation. If magnetism be limited to the attraction of soft iron, then the above conclusion would be correct; but it is not so limited. Plücker calls the attraction of his watch-glass magnetism, the attraction of a salt of iron bears the same name, and it so happens that the attraction of a salt of iron on approaching the poles increases incomparably more slowly than the attraction of iron itself. The proof of this remarkable fact I will now proceed to furnish.

From one end of a very fine balance a sphere of soft iron, $\frac{1}{4}$th of an inch in diameter was suspended. Underneath, and about $\frac{1}{8}$th of an inch distant from the ball when the balance stood horizontal, was the flat end of a straight electro-magnet. On

sending a current of 30° through the surrounding helix, the ball was attracted, and the force necessary to effect a separation was measured: it amounted to 90 grammes. A plate of thin window-glass was then placed upon the end of the magnet, and the ball allowed to rest upon it. The weight necessary to effect a separation, when the magnet was excited by the same current, amounted to 1 gramme. Here an interval of about $\frac{1}{15}$th of an inch was sufficient to reduce the attractive force to $\frac{1}{90}$th of that exerted in the case of contact.

A sphere of sulphate of iron, of somewhat greater diameter than the iron ball, was laid upon one end of the torsion-balance; the opposite core was excited by a current of 30°, and the force necessary to effect a separation of the core and the sphere was determined: it amounted to 20° degrees of torsion. The same plate of glass used in the last experiment was placed against the core end, and the force necessary to effect a separation from it with a current of 30° was also determined. The difference, which in the case of the soft iron amounted to $\frac{89}{90}$ths of the primitive attraction, was here scarcely appreciable. At a distance of $\frac{1}{15}$th of an inch the sphere of sulphate of iron was almost as strongly attracted as when in immediate contact.

Similar experiments were made with a pellet of carbonate of iron, and with the same result. At a distance of $\frac{1}{7}$th of an inch the attraction was two-thirds of that exerted in the case of contact. An interval of $\frac{1}{1000}$th of an inch is more than sufficient to effect a proportionate diminution in the case of soft iron.

A salt of iron in the immediate neighbourhood of the poles behaves like iron itself at a considerable distance, and the deportment of bismuth is exactly similar. A slight change of position will make no great difference of attraction in the one case or of repulsion in the other. To make the antithesis between magnetism and diamagnetism perfect, we require a yet undiscovered metal, which shall bear the same relation to bismuth, antimony, sulphur, &c., which iron does to a salt of iron. Whether nature has such a metal in store for the enterprising physicist, is a problem on which I will hazard no conjecture.

PRINCIPAL RESULTS OF THE FOREGOING INVESTIGATION.

1. *The repulsion of a diamagnetic substance placed at a fixed distance from the pole of a magnet is governed by the same law as the attraction of a magnetic substance.*

2. *The entire mass of a magnetic substance is most strongly attracted when the attracting force acts parallel to that line which sets axial when the substance is suspended in the magnetic field; and the entire mass of a diamagnetic substance is most strongly repelled when the repulsion acts parallel to the line which sets equatorial in the magnetic field.*

3. *The superior attraction and repulsion of the mass in a particular direction is due to the fact, that in this direction the material particles are ranged more closely together than in other directions; the force exerted being attractive or repulsive according as the particles are magnetic or diamagnetic. This is a law applicable to matter in general, the phenomena exhibited by crystals in the magnetic field being particular manifestations of the same.*

BERLIN: June, 1851.

ADDITIONS AND REMARKS, 1870.

Poisson's prediction of Magne-crystallic action.

In March 1851, Professor, now Sir William Thomson, drew attention to an exceedingly remarkable instance of theoretic foresight on the part of Poisson, with reference to the possibility of magne-crystallic action.

'Poisson,' says Sir William, 'in his mathematical theory of magnetic induction, founded on the hypothesis of magnetic fluids, "moving within the infinitely small magnetic elements," of which he assumes magnetisable matter to be constituted, does not overlook the possibility of those magnetic elements being non-spherical and symmetrically arranged in crystalline matter, and he remarks that a finite spherical portion of such a substance would, when in the neighbourhood of a magnet, act differently according to the different positions into which it

might be turned with its centre tube fixed. But "such a circumstance not having yet been observed," he excludes the consideration of the structure which would lead to it from his researches, and confines himself in his theory of magnetic induction to the case of matter consisting either of spherical magnetic elements or of non-symmetrically disposed elements of any forms. Now, however, when a recent discovery of Plücker's has established the very circumstance, the observation of which was wanting to induce Poisson to enter upon a full treatment of the subject, the importance of working out a magnetical theory of magnetic induction is obvious.'

Sir William Thomson then proceeds to make the necessary 'extension of Poisson's mathematical theory of magnetic induction;' and he publishes the following striking quotation :—

'La forme des élémens pourra aussi influer sur cette intensité; et cette influence aura cela de particulier, qu'elle ne sera pas la même en des sens différens. Supposons, par exemple, que les élémens magnétiques sont des ellipsoïdes dont les axes ont la même direction dans toute l'étendue d'un même corps, et que ce corps est une sphère aimantée par influence, dans laquelle la force coercitive est nulle; les attractions ou répulsions qu'elle exercera au dehors seront différentes dans le sens des axes de ces élémens et dans tout autre sens; en sorte que si l'on fait tourner cette sphère sur elle-même, son action sur un même point changera, en général, en grandeur et en direction. Mais si les élémens magnétiques sont des sphères de diamètres égaux ou inégaux, ou bien s'ils écartent de la forme sphérique, mais qu'ils soient disposés sans aucune régularité dans l'intérieur d'un corps aimanté par influence, leur forme n'influerait plus sur les résultats, qui dépendront seulement de la gourme de leurs volumes, comparée au volume entier de ce corps, et qui seront alors les mêmes en tout sens. Ce dernier cas est celui du fer forgé, et sans doute aussi des autres corps non cristallisés dans lesquels on a observé le magnétisme. Mais il serait curieux de chercher si le premier cas n'aurait pas lieu lorsque ces substances sont cristallisées; on pourrait s'assurer par l'expérience soit en approchant un cristal d'une aiguille aimantée, librement suspendue, soit en faisant osciller de petites aiguilles taillées dans des cristaux en toute sorte de sens, et soumises à l'action d'un très-fort aimant.' (Mém. de l'Institut, 1821–22. Paris, 1826.)

Subsequent to the foregoing inquiries, I had a powerful and delicate torsion balance constructed for me by Mr. Becker, and in the autumn of 1855, I examined with it the differential attractions and repulsions of large additional number of crystals and compressed substances.

Dichroite was one of the crystals then examined. It was magnetic. The form was a cube with two pairs of faces parallel to the crystallographic axis, and one pair perpendicular to it. The crystal was found to possess three magnetic axes of unequal values. Measured twice in each case by the torsion balance the attraction of the mass along the three axes respectively was—

	Least axis.	Middle axis.	Greatest axis.
	222	293	300
	225	288	300
Mean . :	223·5	290·5	300

When the crystal was suspended from its centre of gravity with the least and greatest axes horizontal, the rapidity of its vibration was greater than when the intermediate axis was pitted against either of the two others. Depending as it did upon the differential induction the rate of vibration ought of course to be highest where the difference is greatest.

Various other crystals possessing three magnetic axes were examined at the time here referred to. The deportment when suspended from their centres of gravity in the magnetic field was always in harmony with the differential attractions and repulsions of the mass as measured by the torsion balance. Numerous compressed substances were also examined, and their deportment on the torsion balance compared with their deportment in the magnetic field. As far as the experiments extended the harmony observed in the case of crystals was exhibited here also.

It would give me great pleasure to go again over the ground traversed in the preceding papers. The experiments, I think, are secure ; but I should like to review the molecular theory of the whole subject, and examine still further the remarkable variations of magnetic capacity produced by mechanical strains and pressures. In 1855 a great number of experiments were made on compressed powders ; but I was deflected from the subject immediately afterwards ; and from 1856 to the present time I have been unable to bestow any attention on the subject

of diamagnetism. A rich reward is probably here in store for the young investigator.

In the foregoing pages, the mutual inductive action of the particles of carbonate of iron is referred to. Their shape ought also to be taken into account. From a long list of experiments I will take one which bears upon this point.

Pure white wax is strongly diamagnetic. When squeezed between clean plates it always sets the line of compression equatorial in the magnetic field.

A crystal of pure carbonate of iron was pounded to an extremely fine powder in a mortar. The finger and thumb were dipped into the mixture, and the powder adhering to them was in great part brushed away by mutual friction. The minute residue was mixed with a quantity of white wax. The mass was then squeezed; square plates were taken from the flattened mass, and laid one upon another to form a cube. Suspended in the magnetic field it set the line of compression axial.

When the smallness of the quantity of magnetic powder here employed and its extremely sparse diffusion in the mass of the wax are taken into consideration, it can hardly be supposed that the setting of the line of compression axial was due to the mutual induction of the particles. It is more probable that the pressure brought the axes of the minute crystals composing the dust into partial parallelism with the line of compression. This would be the natural result of the shape of the particles. The longest dimension will tend to set perpendicular to the direction of pressure, and this, in the particular case before us, would bring the direction of maximum magnetisation parallel to the same line. The surmise of Sir William Thomson would thus be justified.

But though this action probably occurs in the case of carbonate of iron, it fails in its application to compressed bismuth crystals. There is nothing in the structure of the crystal to warrant the notion that the effect of compression is merely to re-arrange the particles. *By mechanical pressure a new magnetic capacity is here superinduced.*

Three other cubes were formed of the wax in the manner above described, the wax being kneaded in the three respective cases with increasing quantities of the carbonate of iron. The mixture was then compressed, and it was found that the

adherence of the line of compression to the line joining the poles became stronger as the quantity of the carbonate of iron dust was increased.

But now a curious effect is to be mentioned which needs further examination. A quantity of very fine oxide of iron was mixed with the powder of the carbonate, and the smallest pinch of the mixture was kneaded into a lump of wax. Cubes were formed of the substance in the usual manner. But while the pure carbonate always caused the line of compression to set axial; the admixture of the oxide entirely changed this deportment, and caused the direction of pressure to set equatorial.

Three other cubes were formed containing gradually increasing quantities of the oxide. In all cases the line of compression set equatorial.

A class of results of which this is a type was forced on my attention by the anomalous behaviour of the carbonate of iron in certain cases. The line of compression sometimes set axial, sometimes equatorial; the discrepancies being finally traced to the oxide which adhered here and there as a crust to the pure crystal. A great number of different powders were thus examined; and, indeed, iron itself reduced to powder in various ways. The greatest difficulty in these experiments arose from the fact that in strongly magnetic substances the slightest elongation of the particle was sufficient to determine its position. The coercive force of all magnetic powders was also a source of confusion and difficulty.

At the time here referred to I also tried various experiments with a view of connecting calorific conduction with magnetic induction. Heat and magnetism do not seem to be operated upon equally by molecular arrangement. By a beautiful and simple mode of experiment, de Senarmont has shown that crystals conduct heat differently in different directions, and one of the best examples of this difference is furnished by rock-crystal. Coating a plate of the substance with wax, and passing through it a heated wire, the heat communicated to the crystal will melt the wax into an oval, the longest axis of which is parallel to the axis of the crystal.* As regards heat the differential action here is specially striking, but hardly any crystal is less inactive

* *Annales de Chimie et de Physique*, vol. xxi. p. 457, also vol. xxii. *Heat as a Mode of Motion*, 3rd edition, page 202.

than quartz in the magnetic field. Hence the state of the ether, or of the molecules, which produces great differences as regards calorific conduction, may produce no sensible difference as regards magnetic induction. Sulphate of baryta has, according to de Senarmont, sensibly the same calorific conductivity in all directions; but it has three unequal axes of magnetic induction; two parallel to the two diagonals of the base, and an intermediate one parallel to the axis of the prism.

The ratio of the two axes of the ellipse in rock-crystal is as 131 : 100; while in calcite, which is far more energetic in the magnetic field, the ratio is only as 111 : 100. In calcite, moreover, the direction of greatest calorific conduction is also that of highest magnetic induction, while in selenite the case is reversed. In transparent tourmaline the direction of minimum calorific conduction is parallel to the axis; this, at all events in coloured magnetic crystals, is the direction of maximum magnetic induction. De Senarmont says, 'It is remarkable to observe that quartz, the optical constants of which differ little among themselves, compared with those of calc-spar, possesses on the contrary conductibilities which differ far more than those of the spar.'* The magnetic deportment of quartz is more analogous to its optical than to its calorific deportment. A similar remark applies to selenite. As soon as I can command the necessary time, I shall examine whether there is any general relation here.

* *Annales de Chimie et de Physique*, vol. xxviii. p. 279.

POLARITY OF THE DIAMAGNETIC FORCE.

Introduction, 1870.

Soon after the discovery of diamagnetism, Professor Reich, of Freiburg, made the following very important experiment. Placing a ball of bismuth on a torsion balance which had been previously employed in determinations of the density of the earth, he found that 'magnet bars, on being brought up in a horizontal direction to the case near the ball, produced a very distinct repulsion, both when the north and the south pole were brought near. But when several similar bars were brought near, half with their north and the other half with their south poles, there was no effect perceptible, or merely a slight one arising from the inequality of the magnets employed.'* Prof. W. Weber† immediately saw the bearing of this result on the character of diamagnetism. 'From this single experiment,' he says, 'it might be concluded with the greatest probability that the origin of the diamagnetic force is not to be sought for in the never-changing metallic particles of the bismuth, but in an imponderable constituent moving between them, which on the approach of the pole of a magnet is displaced and distributed differently according to the difference of this pole.' He then inquires into the nature of this imponderable constituent, and into its bearing on the view first enunciated by Faraday, that diamagnetism might be explained by assuming the existence of a polarity the reverse of that of magnetism. He subjects the view to an experimental test, and shows that a bar of bismuth which at a certain distance had no sensible action on a magnetic needle, did exert an action on the same needle when

* Poggendorff's *Annalen*, vol. lxxiii. p. 60; Phil. Mag. vol. xxxiv. p. 127.

† Poggendorff's *Annalen*, January 7, 1848; Taylor's *Scientific Memoirs*, vol. v p. 477.

placed between the poles of a powerful magnet.* 'Between the two poles of the horseshoe magnet,' writes Weber, 'a very perceptible and measurable effect is exhibited, viz., a deflection of the needle, owing to one pole being repelled and the other attracted.' He found that when the poles of the influencing magnet were reversed, the deflection produced by the bismuth was reversed also; and that when a piece of iron was substituted for the bismuth, the deflection produced by the magnetic metal was opposite to that produced by the diamagnetic one. Hence he concluded that Faraday's hypothesis was proved. To render the proof more complete, Weber made an exceedingly skilful arrangement to show that induced currents were excited by the diamagnetisation of bismuth as well as by the magnetisation of iron. The proof of diamagnetic polarity appeared, therefore, to be complete.

Faraday, however, again took up the subject. Referring to his hypothesis of diamagnetic polarity, he says the view was 'received so favourably by Plücker, Reich, and others, but above all by W. Weber, that I had great hope it would be confirmed; and though certain experiments of my own did not increase that hope, still my desire and expectation were in that direction.' 'It appeared to me,' he continues, 'that many of the results which have been supposed to indicate a polar condition, were only consequences of the law that diamagnetic bodies tend to go from stronger to weaker places of magnetic action.' In a paper of great experimental power, he demonstrates that the induced currents ascribed by Weber to the diamagnetisation of bismuth were probably due to a totally different cause; and with regard to Weber's experiment with the bar of bismuth placed between the poles of a magnet, Faraday says, 'I have repeated this experiment most anxiously and carefully, but have never obtained the slightest trace of action with the bismuth. I have obtained action with the iron; but in those cases the action was far less than if the iron were applied outside, between the horseshoe magnet and the needle, or to the needle alone, the magnets being entirely away. On using a garnet, or a weak diamagnetic substance of any kind, I cannot

* The action of the magnetic poles upon the suspended needle was neutralised by a second magnet, the needle being thus rendered sufficiently sensitive to respond to the action of the bismuth.

find that the arrangement is at all comparable, for readiness of indication or delicacy, with the use of a common or an astatic needle, and therefore I do not understand how it could become a test of the polarity of bismuth when these fail to show it.'

'Finally,' he continues, 'I am obliged to say that I can find no experimental evidence to support the hypothetical view of diamagnetic polarity, either in my own experiments, or in the repetition of those of Weber, Reich, or others. I do not say that such a polarity does not exist, and I should think it possible that Weber, by far more delicate apparatus than mine, had obtained a trace of it, were it not that then also he would have certainly met with the far more powerful effects produced by copper, gold, silver, and the better conducting diamagnetics.'

In a very exhaustive and beautiful memoir translated by myself from Poggendorff's Annalen, vol. lxxxvii., p. 145,* Prof. Weber returns to the subject of diamagnetism, and considers four possible assumptions to account for the origin of the diamagnetic effects:—

1. The internal cause of such effects may be referred to the existence of two magnetic fluids which are more or less independent of the ponderable matter which carries them.

2. They may be due to the existence of two magnetic fluids, which are only capable of moving in connexion with their ponderable carriers (rotatory molecular magnets).

3. They may be due to the existence of permanent molecular currents formed by the electric fluids, and which rotate with the molecules.

4. They may be due to the existence of electric fluids, which can be thrown into molecular currents.

Weber decides in favour of the fourth hypothesis. He supposes that by the act of magnetisation molecular currents are *generated* in diamagnetic bodies; which currents, like those of Faraday, have a direction opposed to that of their generators. But Faraday's currents are of vanishing duration, being immediately extinguished by the resistance of the conductors through which they move. Diamagnetism, however, would require *permanent* molecular currents to account for it. Weber

* *Scientific Memoirs*, published by Taylor & Francis, New Series, vol. i. p. 163.

secures this permanence by supposing that the induced molecular currents move *in channels of no resistance** round the molecules. This assumption enables him to link all the phenomena of diamagnetism together in a satisfactory manner. While recognising the extreme beauty of the hypothesis, I should hesitate to express a belief in its truth.

Weber also again applied his wonderful experimental skill to the subject of currents induced by the act of diamagnetisation; and in my opinion, fairly met all the requirements of the case; but neither his labours nor those of Poggendorff and Plücker produced conviction in the mind of Faraday. The notion of a distinct diamagnetic polarity was also opposed by others. Prof. von Feilitzsch, for example, contended, on theoretic grounds, and backed his contention by definite experiments, that the magnetic excitement of bismuth and of iron were one and the same. This was also the view of M. Becquerel. Matteucci subsequently entered the field as an ardent opponent of diamagnetic polarity.

The following investigations bear upon this subject.

* This, indeed, is involved in Ampère's theory of molecular currents. See Letter of Prof. Weber further on.

THIRD MEMOIR.

ON THE POLARITY OF BISMUTH, INCLUDING AN EXAMINATION OF THE MAGNETIC FIELD.*

The polarity of bismuth is a subject on which philosophers have differed and on which they continue to differ. On the one side we have Weber, Poggendorff, and Plücker, each affirming that he has established this polarity; on the other side we have Faraday, not affirming the opposite, but appealing to an investigation which is certainly calculated to modify whatever conviction the results of the above-named experimenters might have created. It will probably have occurred to those occupied experimentally with diamagnetic action that whenever the simple mode of permitting the body experimented with to rotate round an axis passing through its own centre of gravity, can be applied, it is preferable in point of delicacy to all others. A crystal of calcareous spar, for example, when suspended from a fine fibre between the poles, readily exhibits its directive action, even in a field of weak power; while to establish that peculiar repulsion of the mass which is the cause of the directive action, even with high power and with the finest torsion balance, is a matter of considerable difficulty. In the knowledge of this and in the fact of my having a piece of bismuth, whose peculiar structure suggested the possibility of submitting the question of diamagnetic polarity to a new test, the present brief enquiry originated.

In December 1847 a paper on 'Diamagnetic Polarity' was read before the Academy of Sciences in Berlin by Professor Poggendorff, the result arrived at by the writer being, that a bismuth bar, suspended horizontally and occupying the equatorial position between two excited magnetic poles, was transversely magnetic—that side of the bar which faced the north pole possessing north polarity, and that side which faced the south pole possessing south polarity; the excitation being thus

* Phil. Mag., Nov. 1851.

the opposite of that of iron, and in harmony with the original conjecture of Faraday.

The method adopted by M. Poggendorff was as follows:—the bismuth bar was suspended within a helix of copper wire, the coils of which were perpendicular to the axis of the bar. The helix was placed between the opposite poles of a magnet, so that the axis of the helix was perpendicular to the line joining the poles. The bismuth took up the usual equatorial position, its length thus coinciding with the axis of the helix. On sending an electric current through the latter the bar was weakly deflected in a certain direction, and on reversing the current, a feeble deflection in the opposite direction was observed. The deflection was such as must follow from the supposition, that the north pole of the magnet had excited a north pole in the bismuth, and the south pole of the magnet a south pole.

It will be at once seen that a considerable mechanical disadvantage was connected with the fact that the distance from pole to pole of the transverse magnet was very short, being merely the diameter of the bar. If a piece of bismuth, instead of setting equatorial, could be caused to set axial, a mechanical couple of far greater power would be presented to the action of the surrounding current. Now it is well known that bismuth sets in the magnetic field with the plane of most eminent cleavage equatorial; hence, if a bar of bismuth could be obtained with the said plane of cleavage perpendicular to its length, the directive power of such a bar might be sufficient to overcome the tendency of its ends to proceed from stronger to weaker places of magnetic action and to set the bar axial. After repeated trials of melting and cooling in the laboratory of Professor Magnus in Berlin, I succeeded in obtaining a plate of this metal in which the plane of most eminent cleavage was perpendicular to the flat surface of the plate, and perfectly parallel to itself throughout. From this plate a little cylinder, an inch long and 0·2 of an inch in diameter, was cut, which being suspended horizontally between the excited poles, turned strongly into the axial position, thus behaving to all appearance as a bar of iron.

About 100 feet of copper wire overspun with silk were wound into a helix so that the cylinder was able to swing freely within it. Through a little gap in the side of the helix a fine silk fibre

descended, to which the bar was attached; to prevent the action of the bar from being disturbed by casual contact with the little fibrous ends protruding from the silk, a coating of thin paper was gummed to the interior.

The helix was placed between the flat poles of an electro-magnet, so that the direction of its coils was from pole to pole. It being first ascertained that the bar moved without impediment, and that it hung perfectly horizontal, the magnet was excited by two of Bunsen's cells; the bar was immediately pulled into the axial line, being in this position parallel to the surrounding coils. A current from a battery of six cells was sent through the helix, so that the direction of the current, *in the upper half of the helix*, was from the south pole to the north pole of the magnet. The cylinder, which an instant before was motionless, was deflected, forming at the limit of its swing an angle of 70° with its former position; the final position of equilibrium for the bar was at an angle of 35°, or thereabouts, with the axial line.

Looking from the south pole towards the north pole of the magnet, or in the direction of the current as it passed *over* the bar, that end of the bar which faced the south pole swung *to the left.*

The current through the helix being interrupted and the bar brought once more to rest in the axial position (which of course is greatly facilitated by the proper opening and closing of the circuit), a current was sent through in the opposite direction, that is from the north pole to the south; the end of the bar, which in the former experiment was deflected to the left, was now deflected an equal quantity to the right. I have repeated this experiment a great number of times and on many different days with the same result.

In this case the direction of the current by which the magnet was excited was constant, that passing through the helix which surrounded the bismuth cylinder being variable. The same phenomena are exhibited if we preserve the latter constant and reverse the former.

A polar action seems undoubtedly to be indicated here; but if a polarity be inferred, it must be assumed that the north pole of the magnet excites a south pole in the bismuth, and the south pole of the magnet a north pole in the bismuth; for by refe-

rence to the direction of the current and the concomitant deflection, it will be seen that the deportment of the bismuth is exactly the same as that which a magnetised needle freely suspended between the poles must exhibit under the same circumstances.

The bar of bismuth was then removed, and a little bar of magnetic shale was suspended in its stead; it set axial. On sending a current through the surrounding helix, it was deflected in the same manner as the bismuth. The piece of shale was then removed and a little bar of iron was suspended within the helix; the residual magnetism which remained in the cores after the cessation of the exciting current was sufficient to set the bar axial; a very feeble current was sent through the helix and the deflection observed—it was exactly the same as that of the bismuth and the shale.

These results being different from those obtained by M. Poggendorff, I repeated his experiment with all possible care. A bar of ordinary bismuth, an inch in length and about 0·2 of an inch in diameter, was suspended within the helix; on exciting the magnet, it receded to the equator, and became finally steady there. The axis of the bar thus coincided with the axis of the helix. A current being sent through the latter, the bar was distinctly deflected. Supposing an observer to stand before the magnet, with the north pole to his right and the south pole to his left, then when a current passed through the upper half of the coil from the north to the south pole, that end of the bismuth which was turned towards the observer was deflected towards the north pole; and on reversing the current, the same end was deflected towards the south pole. This seems entirely to agree with the former experiment. When the bar hung equatorially between the excited poles, on the supposition of polarity the opposite ends of all its horizontal diameters were oppositely polarised. Fixing our attention on one of these diameters, and supposing that end which faced the north pole of the magnet to be gifted with south polarity, and the end which faced the south pole endowed with north polarity, we see that the deportment to be inferred from this assumption is the same as that actually exhibited; for the deflection of *a polarised diameter* in the same sense as a magnetic needle, is equivalent to the motion of *the end of the bar* observed in the experiment.

The following test, however, appears to be more refined

than any heretofore applied. Hitherto we have supposed the helix so placed between the poles that the direction of its coils was parallel to the line which united them; let us now suppose it turned 90° round, so that the axis of the helix and the line joining the poles may coincide. In this position the planes of the coils are parallel to the planes in which, according to the theory of Ampère, the molecular currents of the magnet must be supposed to move; and we have it in our power to send a current through the helix in the same direction as these molecular currents, or in a direction opposed to them. Supposing the bar first experimented with suspended within the coil and occupying the axial position between the excited poles, a current in the helix opposed to the molecular currents of the magnet will, according to the views of the German philosophers named at the commencement, be in the same direction as the currents evoked in the bismuth: hence such a current ought to exert no deflecting influence upon the bar; its tendency, on the contrary, must be to make the bar more rigid in the axial position. A current, on the contrary, whose direction is the same as that of the molecular currents in the magnet, will be opposed to those evoked in the bismuth; and hence, under the influence of such a current, the bar ought to be deflected.

The bar first experimented with was suspended freely within the helix, and permitted to come to rest in the axial position. A current was sent through the helix in the same direction as the molecular currents of the magnet, but not the slightest deflection of the bar was perceptible; when, however, the current was sent through in the opposite direction, a very distinct deflection was the consequence: by interrupting the current whenever the bar reached the limit of its swing, and closing it when the bar crossed the axial line, the action could be increased to such a degree as to cause the bar to make an entire rotation round the axis of suspension. This result is diametrically opposed to the above conclusion [as to diamagnetic polarity]—here again the bismuth bar behaves like a bar of iron.

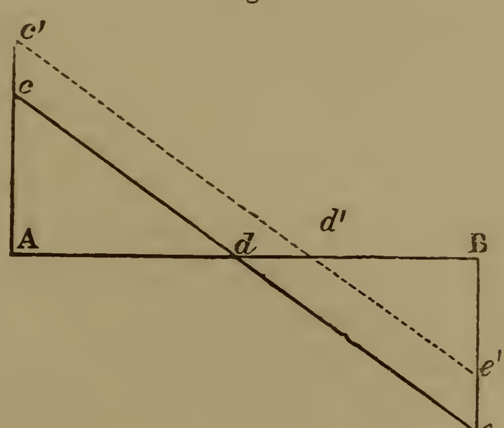
These experiments seem fully to bear out the theory advanced by M. von Feilitzsch in his letter to Mr. Faraday.* He endeavours to account for diamagnetic action on the hypothesis that its polarity is the same as that of iron; 'only with this difference, that in a bar of magnetic substance the intensity of the

* Phil. Mag., S. 4. vol. i. p. 46.

distribution over the molecules *increases* from the ends to the middle, while in a bar of diamagnetic substance it *decreases* from the ends to the middle.' So far as I can see, however, the reasoning of M. von Feilitzsch necessitates the assumption, that in the self-same molecule the poles are of unequal values, that the intensity of the one is greater than that of the other, an assumption which will find some difficulty of access into the speculations of most physicists. A peculiar *directive* action might be readily brought about by the distribution of magnetism assumed by M. von Feilitzsch; but up to the present time I see no way of reconciling the repulsion of the total mass of a piece of bismuth with the idea of a polarity similar to that of iron.

During these inquiries, an observation of Mr. Faraday perpetually recurred to me. 'It appeared to me,' he writes,* 'that many of the results which had been supposed to indicate a polar condition were only consequences of the law that diamagnetic bodies tend to go from stronger towards weaker places of action.' The question here arose, whether the various actions observed might not be explained by reference to the change effected in the magnetic field when it is intersected by an electric current. The distribution of magnetic intensity between the poles will perhaps be rendered most clear by means of a diagram. Let AB represent the distance between the polar faces; plotting the intensity at every point in AB as an ordinate from that point, the line which unites the ends of all these ordinates will express the magnetic distribution. Suppose this line to be *cde*. Commencing at A, the intensity of attraction towards this face decreases as we approach the centre *d*, and at this point it is equilibrated by the equal and opposite attraction towards B. Beyond *d* the residual attraction towards A becomes negative, that is, it is now in the direction of *d*B. The point *d* will be a position of stable equilibrium

Fig. 1.

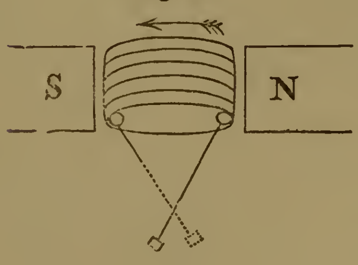


* Phil. Mag., S. 3. vol. xxxvii. p. 89.

for a diamagnetic sphere, and of unstable equilibrium for a magnetic sphere. But if, through the introduction of some extraneous agency, the line of distribution be shifted, say to *c′d′e′*, the point will be no longer a position of equilibrium; the diamagnetic sphere will move from this point to *d′*, and the magnetic sphere will move to the pole A.

For the purpose of investigating whether any change of this nature takes place in the magnetic field when an electric current passes through it, I attached a small sphere of carbonate of iron to the end of a slender beam of light wood; and balancing it by a little copper weight fixed to the other end, suspended the beam horizontally from a silk fibre. Attaching the fibre to a movable point of suspension, the little sphere could be caused to dip into the interior of the helix as it stood between the poles, and to traverse the magnetic field as a kind of feeler. The law of its action being that it passes from weaker to stronger places of force, we have in it a ready and simple means of testing the relative force of various points of action. The point of the beam to which the fibre was attached being cut by the axis of the helix produced, and the sphere being also on the same level with the axis, when the magnet was excited* it passed into the position occupied by the *defined* line in fig. 2, thus resting against the interior of the helix a little within its edge. On sending a current through the helix, which in the upper half thereof had the direction of the arrow, the sphere loosed from its position, sailed gently across the field, and came to rest in the position of the dotted line. If, while thus sailing, the direction of the current in the helix, or of the current by which the magnet was excited, were reversed, the sphere was arrested in its course and brought back to its original position. In like manner, when the position of the sphere between the poles was that of the dotted line, a current sent through the helix in a direction opposed to the arrow, caused the sphere to pass over into the position of the defined line.

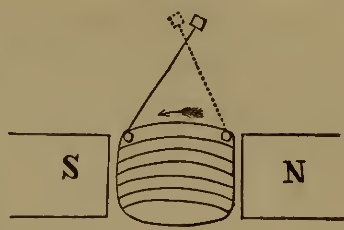
Fig. 2.



* One of Bunsen's cells was found sufficient; when the magnetic power was high, the change caused by the current was not sufficient to deflect the beam.

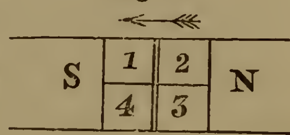
The sphere was next introduced within the opposite edge of the helix (fig. 3). On exciting the magnet, the beam came to rest in the position of the defined line; on sending a current through the helix in the direction of the arrow, the sphere loosed, moved towards the north pole, and came to rest in the dotted position. If while in this position either the current of the magnet or the current of the helix were reversed, the sphere went back; if both were reversed simultaneously, the sphere stood still.

Fig. 3.



From these facts we learn, that if the magnetic field be divided into four compartments, as in fig. 4, the passage of an electric current through a helix placed therein, the direction of the current in the upper half of the helix being that indicated by the arrow, will weaken the force in the first and third quadrants, but will strengthen it in the second and fourth. With the aid of this simple fact we can solve every experiment made with the bismuth bars. For instance, it was found that when an observer stood before the magnet with a north pole to his right and a south pole to his left, a current passing through the upper half of the helix from the north to the south pole deflected a bar of ordinary bismuth, which had previously stood equatorial, so that the end presented to the observer moved towards the north pole. This deportment might be inferred from the constitution of the magnetic field; the bar places its ends in quadrants 1 and 3, that is, in the positions of weakest force.

Fig. 4.

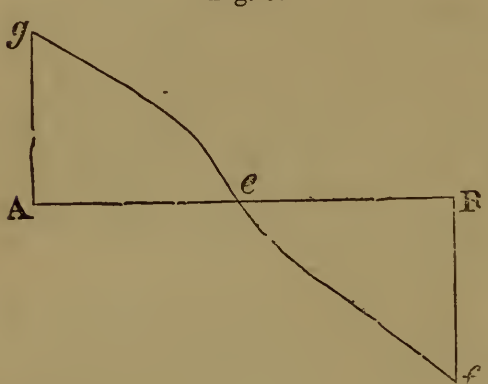


The experiments with the other bar are capable of an explanation just as easy. Preserving the arrangement as in the last figure, the bismuth bar, which previously stood axial, would be deflected by the surrounding current, so that its two ends would occupy the quadrants 2 and 4, that is, the positions of strongest force. Now this is exactly what they did in the magnetic field before the passage of any current, for the bar set axial. It was first proved by Mr. Faraday, that the mass of a bismuth crystal was most strongly repelled when the repulsive force acted parallel to the planes of most eminent cleavage; and in the magnetic field the superior repulsion of these planes causes

them always to take up that position where the force is a minimum. It is the equatorial setting of these planes which causes the bar at present under consideration to set axial. The planes of cleavage being thus the true indicators, we see that when these set from the first to the third quadrant, or in the line of weakest action, the ends of the bar must necessarily occupy the second and fourth, which is the deportment observed.

The little test-sphere can also be made available for examining the change brought about in the magnetic field by the introduction of a small bar of iron, as in the experiment of M. Plücker quoted by Mr. Faraday.* Removing the helix from the magnetic field, the little sphere was at liberty to traverse it from wall to wall. When the magnet was excited, the sphere passed slowly on to the pole to which it was nearest and came to rest against it. When forcibly brought into the centre of the magnetic field, after a moment's apparent hesitation it passed to one pole or the other with a certain speed; but when a bar of iron was brought underneath while it was central, this speed was considerably increased. Over the centre of the bar there was a position of unstable equilibrium for the sphere, from which it passed right or left, as the case might be, with greatly increased velocity. The distribution of the force appears in this case to have undergone a change represented by the line *gef* in the diagram. From the centre towards the poles the tension steepens suddenly, the quicker recession of a bismuth bar towards the equator, as observed by M. Plücker, being the natural consequence.

Fig. 5.



Assuming the law of action for a small magnetic sphere to be that it proceeds from weaker to stronger places of force, we find that the passage of an electric current in the manner described so modifies the 'field' [between flat poles], that the positions of its two diagonals are of unequal values as regards the distribution of the force, the position of the field inter-

* Phil. Mag., S. 3. vol. xxxvii. p. 104.

sected by the diagonal which bisects 1 and 3, fig. 4, being weaker than the portion intersected by the diagonal which bisects 2 and 4. But here the believer in diamagnetic polarity may enter his protest against the use which we have made of the assumption. 'I grant you,' he may urge, 'that in a simple magnetic field, consisting of the space before and around a single pole, what you assume is correct, that a magnetic sphere will pass from weaker to stronger places of action; but for a field into which several distinct poles throw their forces, the law by no means sufficiently expresses the state of things. If we place together two poles of equal strengths but of opposite qualities close to a mass of iron, it is an experimental fact that there is almost no attraction; and if they operate upon a mass of bismuth, there is no repulsion. Why? Do the magnetic rays, to express the thing popularly, annul each other by a species of interference *before they reach the body*; or does one pole induce in a body the certain condition upon which the second pole acts in a sense contrary to the former, both poles thus exactly destroying each other? If the former, then I grant you that the magnetic field is rendered weaker, nay deprived of all force if you will, by the introduction of the second pole; but if the latter, then we must regard the field as possessing two systems of forces; and it is to the peculiar inductive property of the body, in virtue of which one system neutralises the other, that we must attribute the absence of attraction or repulsion. Once grant this, however, and the question of diamagnetic polarity, so far as you are concerned, is settled in the affirmative.'

Our hypothetical friend mentions it as 'an experimental fact,' that if dissimilar poles of equal strengths operate upon a mass of bismuth there is no repulsion. This is Reich's result —a result which I have carefully tested and corroborated. I shall now proceed to show the grounds which the believer in diamagnetic polarity might urge in support of his last assertion. A twelve-pound copper helix was removed from the limb of an electro-magnet and set upright. A magnetised sewing-needle being suspended from one end, the other end was caused to dip into the hollow of the spiral, and to rest against its interior surface. When a current was sent through the helix in a certain direction, the needle was repelled towards the axis of the coil; the same end of the needle, when suspended at half an

inch distance from the *exterior* surface of the coil, was drawn strongly up against it. When the current was reversed, the end of the needle was attracted to the interior surface of the coil, but repelled from its exterior surface. If we suppose a little mannikin swimming along in the direction of the current, with his face towards the axis of the helix, the exterior surface of that end towards which his *left arm* would point *repels* the north pole of a magnetic needle, while the interior surface of the same end *attracts* the north pole of a magnetic needle. The complementary phenomena were exhibited at the other end of the helix. Thus if we imagine two observers placed, the one within and the other without the coil, the same end thereof would be a north pole to the one and a south pole to the other.

If we apply these facts to the case of the helix within the magnetic field, we see that each pole of the magnet had two contrary poles of the helix in contact with it; and we moreover find that the quadrants which we have denominated the strongest are those in which the poles of magnet and helix were in conjunction; while the quadrants which we have called weakest are those in which the poles of magnet and helix were in opposition.

'Which will you choose?' demands our hypothetical friend; 'either you must refer the weakening of a quadrant to magnetic interference, or you must conclude, that that induced state, whatever it be, which causes the bismuth to be repelled by the magnet, causes it to be *attracted* by the coil, the resultant being the difference of both forces. In the same manner the strengthening of a quadrant is accounted for by the fact, that here the induced state which causes the bismuth to be repelled by the magnet causes it to be repelled by the coil also, the resultant being the sum of both forces. The matter may be stated still more distinctly by reference to Reich's experiments.* He found that when a bundle of magnet-bars was brought to bear upon a diamagnetic ball suspended to the end of a torsion balance, when similar poles were presented to the body, there was a very distinct repulsion; but if one half of the poles were north and the other half south, there was no repulsion. Let us imagine the two halves to be brought to bear upon the ball consecutively; the first half will cause it to recede to a

* Phil. Mag., S. 3. vol. xxxiv. p. 127.

certain distance; if the second unlike half be now brought near, the ball will approach again, and take up its original position. The question therefore appears to concentrate itself into the following:—Is this "approach" due to the fact that the magnetic forces of the two halves annul each other before they reach the ball, or is it the result of a compensation of inductions in the diamagnetic body itself? If a sphere of soft iron be suspended from a thread, the north pole of a magnet will draw it from the plumb-line; if the south pole of an exactly equal magnet be brought close to the said north pole, the sphere will recede to the plumb-line. Is this recession due to a compensation of inductions in the sphere itself, or is it not? If the former, then, by all parity of reasoning, we must assume a similar compensation on the part of the bismuth.'

That bismuth, and diamagnetic bodies generally, suffer induction, will, I think, appear evident from the following considerations. The power of a magnet is practically ascertained by the mechanical effect which it is able to produce upon a body possessing a constant amount of magnetism,—a hard steel needle, for instance. The action of a magnet in pulling such a needle from the magnetic meridian may be expressed by a weight which acts at the end of a lever of a certain length. By easy practical rules we can ascertain when the pull of one magnet is twice or half the pull of another, and in such a case we should say that the former possesses twice or half the strength of the latter. If, however, these two magnets, with their powers thus fixed, be brought to bear upon a sphere of soft iron, the attraction of the one will be four times or a quarter that of the other. The strengths of the magnets being, however, in the ratio of 1 : 2, this attraction of 1 : 4 can only be explained by taking into account the part played by the iron sphere. We are compelled to regard the sphere as an induced magnet, whose power is directly proportional to the inducing one. Were the magnetism of the sphere a constant quantity, a magnet of double power could only produce a double attraction; but the fact of the magnetism of the sphere varying directly as the source of induction leads us inevitably to the law of squares; and conversely, the law of squares leads us to the conclusion that the sphere has been induced.

These sound like truisms; but if they be granted, there

is no escape from the conclusion that diamagnetic bodies are induced; for it has been proved by M. E. Becquerel and myself, that the repulsion of diamagnetic bodies follows precisely the same law as the attraction of magnetic bodies; the law of squares being true for both. Now were the repulsion of bismuth the result of a force applied to the mass alone, without induction, then, with a constant mass, the repulsion must be necessarily proportional to the strength of the magnet. But it is proportional to the square of the strength, and hence must be the product of induction.

In order to present magnetic phenomena intelligibly to the mind, a material imagery has been resorted to by philosophers. Thus we have the 'magnetic fluids' of Poisson and the 'lines of force' of Mr. Faraday. For the former of these Professor W. Thomson has recently substituted an 'imaginary magnetic matter.' The distribution of this 'matter' in a mass of soft iron, when operated on by a magnet, has attraction for its result. We have the same necessity for an image in the case of bismuth. If we imagine the two magnetic matters which are distributed by induction on a piece of iron to change places, we have a distribution which will cause the phenomena of bismuth. Hence it is unnecessary to assume the existence of any *new* matter in the case of diamagnetic bodies, the deportment being accounted for by reference to a peculiarity of distribution. Further, the experiments of Reich, which prove that the matter evoked by one pole will *not* be repelled by an unlike pole, compel us to assume the existence of *two kinds* of matter, and this, if I understand the term aright, is polarity.

[The foregoing slight paper could have very little influence on the decision of so weighty a question. In the autumn of 1854 I therefore resumed the investigation with a desire to exhaust, if possible, the experimental portion of it. The following memoir contains an account of the inquiry. I had previously been examining the influence of organic structure upon the display of magnetism; and had also been engaged with certain laws deduced by M. Plücker from his experiments as to the diminution of magnetism and diamagnetism with the distance. The account of these experiments precedes the real inquiry into the relations of magnetism to diamagnetism, and ought, perhaps, to have been published by itself.—J. T., 1870.]

FOURTH MEMOIR.

ON THE NATURE OF THE FORCE BY WHICH BODIES ARE REPELLED FROM THE POLES OF A MAGNET.*

Introduction.

From the published account of his researches it is to be inferred, that the same heavy glass, by means of which he first produced the rotation of the plane of polarisation of a luminous ray, also led Mr. Faraday to the discovery of the diamagnetic force. A square prism of the glass, 2 inches long and 0·5 of an inch thick, was suspended with its length horizontal between the two poles of a powerful electro-magnet: on developing the magnetism the prism moved round its axis of suspension, and finally set its length at right angles to a straight line drawn from the centre of one pole to that of the other. A prism of ordinary magnetic matter, similarly suspended, would, as is well known, set its longest dimension from pole to pole. To distinguish the two positions here referred to, Mr. Faraday introduced two new terms, which have since come into general use: he called the direction parallel to the line joining the poles, the *axial* direction, and that perpendicular to the said line, the *equatorial* direction.

The difference between this new action and the ordinary magnetic action was further manifested when a fragment of the heavy glass was suspended before a single magnetic pole: the fragment was repelled when the magnetism was excited; and to the force which produced this repulsion Mr. Faraday gave the name of *diamagnetism*.

Numerous other substances were soon added to the heavy glass, and, among the metals, it was found that bismuth possessed the new property in a comparatively exalted degree. A fragment of this substance was forcibly repelled by either of the poles of a magnet; while a thin bar of the substance, or a glass

* Phil. Trans. 1855, p. 1: being the Bakerian Lecture.

tube containing the bismuth in fragments, or in powder, suspended between the two poles of a horseshoe magnet, behaved exactly like the heavy glass, and set its longest dimension equatorial.

These exhaustive researches, which rendered manifest to the scientific world the existence of a pervading natural force, glimpses of which merely had been previously obtained by Brugman and others, were made public at the end of 1845; and in 1847 M. Plücker announced his beautiful discovery of the action of a magnet upon crystallised bodies. His first result was, that when any crystal whatever was suspended between the poles of a magnet, with its optic axis horizontal, a repulsive force was exerted on the axis, in consequence of which it receded from the poles and finally set itself at right angles to the line joining them. Subsequent experiments, however, led to the conclusion, that the axes of optically negative crystals only experienced this repulsion, while the axes of positive crystals were attracted; or, in other words, set themselves from pole to pole. The attraction and repulsion, here referred to, were ascribed by M. Plücker to the action of a force, independent of the magnetism or diamagnetism of the mass of the crystal.*

Shortly after the publication of M. Plücker's first memoir,

* '*The force which produces this repulsion is independent of the magnetic or diamagnetic condition of the mass of the crystal; it diminishes less, as the distance from the poles of the magnet increases, than the magnetic and diamagnetic forces emanating from these poles and acting upon the crystal.*'—Prof. Plücker in Poggendorff's *Annalen*, vol. lvii. No. 10; Taylor's Scientific Memoirs, vol. v. p. 353.

The forces emanating from the poles of a magnet are thus summed up by M. Plücker:—

1st. The magnetic force in a strict sense.

2nd. The diamagnetic action discovered by Faraday.

3rd. The action exerted on the optic axes of crystals (and that producing the rotation of the plane of polarisation which probably corresponds to it). *The second diminishes more with the distance than the first, and the first more than the third.*—Taylor's Scientific Memoirs, vol. v. p. 380.

The crystal (cyanite) does not point according to the magnetism of its substance, *but only in obedience to the magnetic action upon its optic axes.*—Letter to Mr. Faraday, Phil. Mag. vol. xxxiv. p. 451. The italics in all cases are M. Plücker's own.

M. de la Rive states the view of M. Plücker to be:—'that the axis in its quality as axis, and independently of the very nature of the substance of the crystal, enjoys peculiar properties, more frequently in opposition to those possessed by the substance itself, or which at least are altogether independent of it.'—Treatise on Electricity, vol. i. p. 359.

Mr. Faraday observed the remarkable magnetic properties of crystallised bismuth; and his researches upon this, and other kindred points, formed the subject of the Bakerian Lecture before the Royal Society for the year 1849.

Through the admirable lectures of Professor Bunsen on Electro-chemistry in 1848, I was first made acquainted with the existence of the diamagnetic force; and in the month of November 1849 my friend Professor Knoblauch, then of Marburg, now of the University of Halle, suggested to me the idea of repeating the experiments of M. Plücker and Mr. Faraday. He had procured the necessary apparatus with the view of prosecuting the subject himself, but the pressure of other duties prevented him from carrying out his intention. I adopted the suggestion and entered upon the inquiry in M. Knoblauch's cabinet. Our frequent conversations upon the subject led naturally to our making a joint investigation. We published our results in two papers, the first of which, containing a brief account of some of the earliest experiments, appeared in the 'Philosophical Magazine' for March 1850, and some time afterwards in Poggendorff's *Annalen*; while the second and principal memoir appeared in the 'Philosophical Magazine' for July 1850, and in Poggendorff's *Annalen* about January 1851.* I afterwards continued my researches in the private laboratory of Professor Magnus of Berlin, who, with prompt kindness and a lively interest in the furtherance of the inquiry, placed all necessary apparatus at my disposal. The results of this investigation are described in a paper published in the 'Philosophical Magazine' for September 1851, and in Poggendorff's *Annalen*, vol. lxxxiii.

In these memoirs it was shown that the law according to which the axes of positive crystals are attracted and those of negative crystals repelled, was contradicted by the deportment of numerous crystals both positive and negative. It was also proved that the force which determined the position of the optic axes in the magnetic field was not independent of the magnetism or diamagnetism of the mass of the crystal; inasmuch as two crystals, of the same form and structure, exhibited alto-

* The memoirs in the 'Philosophical Magazine' were written by me, and the second one has, I believe, been translated into German by Dr. Krönig; the papers in Poggendorff's *Annalen* were edited by my friend and colleague.—J. T.

gether different effects, when one of them was magnetic and the other diamagnetic. It was shown, for example, that pure carbonate of lime was diamagnetic, and always set its optic axis equatorial; but that when a portion of the calcium was replaced by an isomorphous magnetic constituent, which neither altered the structure nor affected the perfect transparency of the crystal, the optic axis set itself from pole to pole. The various complex phenomena exhibited by crystals in the magnetic field were finally referred to the modification of the magnetic and diamagnetic forces by the peculiarities of molecular arrangement.

This result is in perfect conformity with all that we know experimentally regarding the connection of matter and force. Indeed it may be safely asserted that every force which makes matter its vehicle of transmission must be influenced by the manner in which the material particles are grouped together. The phenomena of double refraction and polarisation illustrate the influence of molecular aggregation upon light. Wertheim has shown that the velocity of sound through wood, *along* the fibre, is about five times its velocity *across* the fibre: De la Rive, De Candolle, and myself have shown the influence of the same molecular grouping upon the propagation of heat. In the first section of the present paper, the influence of the molecular structure of wood upon its magnetic deportment is described: De Senarmont has shown that the structure of crystals endows them with different powers of calorific conduction in different directions: Knoblauch has proved the same to be true, with regard to the transmission of radiant heat: Wiedemann finds the passage of frictional electricity along crystals to be affected by structure; and some experiments, which I have not yet had time to follow out, seem to prove, that bismuth may, by the approximation of its particles, be caused to exhibit, in a greatly increased degree, those singular effects of induction which are so strikingly exhibited by copper, and other metals of high conducting power.

Indeed the mere *à priori* consideration of the subject must render all the effects here referred to extremely probable. Supposing the propagation of the forces to depend upon a subtle agent, distinct from matter, it is evident that the progress of such an agent from particle to particle must be influenced by

the manner in which these particles are arranged. If the particles be twice as near each other in one direction as in another, it is certain that the agent of which we speak will not pass with the same facility in both directions. Or supposing the effects to which we have alluded to be produced by motion of some kind, it is just as certain that the propagation of this motion must be affected by the manner in which the particles which transmit it are grouped together. Whether, therefore, we take the old hypothesis of imponderables or the new, and more philosophic one, of modes of motion, the result is still the same.

If this reasoning be correct, it would follow that, if the molecular arrangement of a body be changed, such a change will manifest itself by an alteration of deportment towards any force operating upon the body: the action of compressed glass upon light, which Wertheim in his recent researches* has so beautifully turned to account in the estimation of pressures, is an illustration in point; and the inference also receives the fullest corroboration from experiments, some of which are recorded in the papers already alluded to, and which show that all the phenomena of magne-crystallic action may be produced by simple mechanical agency. What the crystalline forces do in one case, mechanical force, under the control of the human will, accomplishes in the other. A crystal of carbonate of iron, for example, suspended in the magnetic field, exhibits a certain deportment: the crystal may be removed, pounded into the finest dust, and the particles so put together that the mass shall exhibit the same deportment as before. A bismuth crystal suspended in the magnetic field, with its planes of principal cleavage vertical, will set those planes equatorial; but where the crystalline planes are squeezed sufficiently together by a suitable mechanical force, this deportment is quite changed, the line which formerly set equatorial now setting axial.†

Thus we find that the influence of crystallisation may be perfectly imitated, and even overcome, by simple mechanical agencies. It would of course be perfectly unintelligible were we to speak of any direct action of the magnetic force upon the

* Phil. Mag. October and November 1854.

† Phil. Mag. vol. ii. Ser. 4. p. 183.

force by which the powdered carbonate of iron, or the solid cube of bismuth, is compressed; such an idea, however, appears scarcely less tenable than another which seems to be entertained by some who feel an interest in this subject; namely, that there is a direct action of the magnet upon the molecular forces which built the crystal. The function of such forces, as regards the production of the effects, is, I believe, *mediate*; the molecular forces are exerted in placing the particles in position, and the subsequent phenomena, whether exhibited in magne-crystallic action, in the bifurcation and polarisation of a luminous ray, or in the modification of any other force transmitted through the crystal, are not due to the action of force upon force, except through the intermediation of the particles referred to.*

The foregoing introductory statement will, perhaps, sufficiently indicate the present aspect of this question. The object I proposed to myself in commencing the inquiry now laid before the Royal Society was to obtain, if possible, clearer notions of the nature of the diamagnetic force than those now prevalent; for though, in the preceding paragraphs, we have touched upon some of the most complex phenomena of magnetism and diamagnetism, and are able to produce these phenomena at will, the greatest diversity of opinion still prevails as to the real relationship of the two forces. The magnetic force, we know, embraces both attraction and repulsion, thus exhibiting that wonderful dual action which we are accustomed to denote by the term polarity. Mr. Faraday was the first who proposed the hypothesis that diamagnetic bodies, operated on by magnetic forces, possess a polarity 'the same in kind as, but the reverse in direction of, that acquired by iron, nickel, and ordinary magnetic bodies under the same circumstances.' † M. W. Weber sought to confirm this hypothesis by a series of experiments, wherein the excitement of the supposed diamagnetic polarity

* The influence of the molecular aggregation probably manifests itself on a grand scale in nature. The Snowdon range of mountains, for example, is principally composed of slate rock, whose line of strike is nearly north and south. The magnetic properties of this rock I find, by some preliminary experiments, to be very different along the cleavage from what they are across it. I cannot help thinking that these vast masses, in their present position, must exert a different action on the magnetic needle from that which would be exerted if the line of strike were east and west.

† Experimental Researches, 2429, 2430.

was applied to the generation of induced currents—apparently with perfect success. Mr. Faraday afterwards showed, and his results were confirmed by M. Verdet, that effects similar to those described by the distinguished German were to be attributed, not to the excitement of diamagnetic polarity, but to the generation of ordinary induced currents in the metallic mass. On the question of polarity Mr. Faraday's results were negative, and he therefore, with philosophic caution, holds himself unpledged to his early opinion. M. Weber, however, still retains his belief in the reverse polarity of diamagnetic bodies, whereas Weber's countryman M. von Feilitzsch, in a series of memoirs recently published in Poggendorff's *Annalen*, contends that the polarity of diamagnetic bodies is precisely the same as that of magnetic ones. In this unsettled state of the question nothing remained for me but a complete examination of the nature of the diamagnetic force, and a thorough comparison of its phenomena with those of ordinary magnetism. This has been attempted in the following pages: with what success it must be left to the reader to decide.

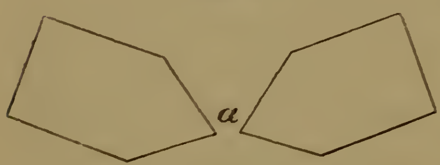
Before entering upon the principal inquiry, I will introduce one or two points which arose incidentally from the investigation, and which appear to be worth recording.

On the Magnetic Properties of Wood.

No experiments have yet been made to determine the influence of structure upon the magnetic deportment of this substance; and even on the question whether it is magnetic, like iron, or diamagnetic, like bismuth, differences of opinion appear to prevail. Such differences are to be referred to the extreme feebleness of the force proper to the wood itself, and its consequent liability to be masked by extraneous impurity. In handling the substance intended for experiment the fingers must be kept perfectly clean, and frequent washing is absolutely necessary. After reducing the substance to a regular shape, so as to annul the influence of exterior form, its outer surface must be carefully removed by glass, and the body afterwards suspended by a very fine fibre between the poles of a strong electromagnet.

The first step in the present inquiry was to ascertain whether the substance examined was paramagnetic* or diamagnetic. It is well known, that, in experiments of this kind, movable masses of soft iron are placed upon the ends of the electro-magnet, the distance between the masses being varied to suit the experiment. In front of a pointed mass of iron of this kind, a cube of wood was suspended; if, on exciting the magnet, the cube was repelled by the point, it was regarded as diamagnetic; if attracted, it was considered to be paramagnetic. The force is considerably intensified by placing the two movable poles as in fig. 1, and suspending the cube at *a* on the same level with the points; a diamagnetic body placed there is, on the development of the magnetic force, forcibly driven *from* the line which unites the points, while a magnetic body is forcibly drawn in between them.

Fig. 1.



Having thus observed the deportment of the mass, the cube was next suspended between the *flat* ends of the poles sketched in fig. 1. The parallel faces were about three-quarters of an inch apart, and in each case the fibre of the suspended wood was horizontal. The specimen first examined was Beef-wood: suspended in the position *a*, fig. 1, the mass was repelled: suspended between the flat poles, on exciting the magnet, the cube, if in an oblique position, turned and set its fibre equatorial. By suitably breaking and closing the circuit the cube could be turned 180° round and held in this new position. The axial position of the ligneous fibre was one of unstable equilibrium, from which, if it diverged in the slightest degree right or left, the cube turned and finally set its fibre equatorial. The following is a statement of the results obtained with thirty-five different kinds of wood:—

* The effects exhibited by iron and by bismuth come properly under the general designation of *magnetic* phenomena: to render their subdivision more distinct Mr. Faraday has recently introduced the word *paramagnetic* to denote the old magnetic effects, of which the action of iron is an example. Wherever the word *magnetic* occurs, without the prefix, it is always the old action that is referred to.

TABLE I.

Name of wood	Deportment of mass	Deportment of structure	Remarks
1. Beef-wood . . .	Diamagnetic	Fibre equatorial	
2. Black Ebony . . .	"	"	
3. Box-wood . . .	"	"	
4. Second specimen . .	"	"	
5. Brazil-wood . . .	"	"	
6. Braziletto . . .	"	"	Action decided
7. Bullet-wood . . .	"	"	Action decided
8. Cam-wood . . .	"	"	
9. Cocoa-wood . . .	"	"	
10 Coromandel-wood . .	"	"	Action strong
11. Green Ebony . . .	"	"	Action strong
12. Green-heart . . .	"	"	Action strong
13. Iron-wood . . .	"	"	
14. King-wood . . .	"	"	Action strong
15. Locust-wood . . .	"	"	
16. Maple	"	"	Action decided
17. Lance-wood . . .	"	"	Action decided
18. Olive-tree . . .	"	"	
19. Peruvian-wood . .	"	"	Action strong
20. Prince's-wood . . .	"	"	
21. Camphor-wood . . .	"	"	
22. Sandal-wood . . .	"	"	
23. Satin-wood . . .	"	"	
24. Tulip-wood . . .	"	"	
25. Zebra-wood . . .	"	"	
26. Botany Bay Oak . .	"	"	Action strong
27. Mazatlan-wood . .	"	"	Action decided
28. Tamarind-wood . .	"	"	
29. Sycamore . . .	"	"	Action decided
30. Beech	"	"	Action decided
31. Ruby-wood . . .	"	"	
32. Jacca	"	"	
33. Oak	"	"	Action strong
34. Yew	"	"	Action feeble
35. Black Oak . . .	Paramagnetic	"	Action decided

The term 'decided' is here used to express an action more energetic than ordinary, but in no case does the result lack the decision necessary to place it beyond doubt. It must also be remarked that the term 'strong' is used in relation to the general deportment of wood; for, compared with the action of many other diamagnetic bodies, the strongest action of wood is but feeble. Simple as the problem may appear, it required considerable time and care to obtain the results here recorded. During the first examination of the cubes eight anomalies presented themselves—in eight cases the fibre set either oblique or axial. The whole thirty-five specimens were carefully rescraped with glass and tested once more; still two remained, which, though repelled as masses, persistently set with the fibre axial,

and oscillated round this position so steadily as to lead to the supposition that the real deportment of the substance was thus exhibited. I scraped these cubes ten times successively, and washed them with all care, but the deportment remained unchanged. The cubes, for the sake of reference, had been stamped with diminutive numbers by the maker of them; and I noticed at length, that in these two cases a trace of the figures remained; on removing the whole surface which bore the stamp from each, the cubes forsook the axial position, and set, like the others, with the fibre equatorial.

The influence of the mere *form* of an impurity was here very prettily exhibited. The cubes in question had been stamped (probably by a steel tool) with the numbers 33 and 37, which lay in the line of the fibre; the figures, being dumpy little ones, caused an elongation of the magnetic impurity along the said line, and the natural consequence of this elongation was the deportment above described.

Of the thirty-five specimens examined one proved to be paramagnetic. Now, it may be asked, if the views of molecular action stated in the foregoing pages be correct, how is it that this paramagnetic cube sets its fibre equatorial? The case is instructive. The substance (bog oak) had been evidently steeped in a liquid containing a small quantity of iron in solution, whence it derived its magnetism; but here we have no substitution of paramagnetic molecules for diamagnetic ones, as in the cases referred to. The extraneous magnetic constituent is practically indifferent as to the direction of magnetisation, and it therefore accommodates itself to the directive action of the wood to which it is attached.

On the Rotation of Bodies between Pointed Magnetic Poles.

In his experiments on charcoal, wood-bark, and other substances, M. Plücker discovered some very curious phenomena of rotation, which occurred on removing the substance experimented on from one portion of the magnetic field to another. To account for these phenomena, he assumed, that in the substances which exhibited the rotation, two antagonist forces were perpetually active—a repulsive force which caused the

substance to assume one position; and an attractive force which caused it to assume a different position: that, of these two forces, the repulsive diminished more quickly than the attractive, when the distance of the body from the poles was augmented. Thus, the former, which was predominant close to the poles, succumbed to the latter when a suitable distance was attained, and hence arose the observed rotation.

Towards the conclusion of the memoir published in the September number of the 'Philosophical Magazine' for 1851, I stated that it was my intention further to examine this highly ingenious theory. I shall now endeavour to fulfil the promise then made, and to state, as briefly as I can, the real law which regulates these complex phenomena.

The masses of soft iron sketched in fig. 1 were placed upon the ends of the electro-magnet, with their points facing each other; between the points the body to be examined was suspended by a fine fibre, and could be raised or lowered by turning a milled head. The body was first suspended on the level of the points and its deportment noted, it was then slowly elevated, and the change of position, if any, was observed. It was finally permitted to sink below the points and its deportment there noted also.

The following is a statement of the results; the words 'equatorial' (E) and 'axial' (A) imply that the longest horizontal dimension of the substance examined took up the position denoted by each of these words respectively. The manner in which the bars were prepared is explained further on.

TABLE II.

Name of substance	Horizontal dimensions	Deportment of mass	Position		
			Between poles	Above	Below
1. Tartaric acid . .	0·5 × 0·1	Diamagnetic	E	A	A
2. A second specimen .	0·4 × 0·1	,,	E	A	A
3. Red ferrocyanide of potassium . .	0·6 × 0·1	Paramagnetic	A	E	E
4. A second prism . .	0·9 × 0·12	,,	A	E	E
5. Citric acid . . .	0·55 × 0·25	Diamagnetic	E	A	A
6. A second specimen .	0·48 × 0·2	,,	E	A	A
7. Beryl . . .	0·45 × 0·1	Paramagnetic	A	E	E
8. Saltpetre . . .	0·6 × 0·3	Diamagnetic	E	A	A
9. Nitrate of soda . .	0·6 × 0·12	,,	E	A	A
10. Sulphate of iron . .	0·7 × 0·15	Paramagnetic	A	E	E
11. A second specimen .	0·6 × 0·03	,,	A	E	E
12. A third specimen .	1·0 × 0 13	,,	A	E	E

TABLE II.—*continued.*

Name of substance	Horizontal dimensions	Deportment of mass	Position: Between poles	Position: Above	Position: Below
13. Calcareous spar . .	0·5 × 0·1	Diamagnetic	E	A	A
14. A full crystal . .	...	"	E	A	A
15. Carbonate of iron .	0·5 × 0·1	Paramagnetic	A	E	E
16. Carbonate of iron powdered and compressed	0·9 × 0·18	"	A	E	E
17. Compressed disc .	0·8 × 0·08	"	A	E	E
18. Bismuth . . .	0·95 × 0·15	Diamagnetic	E	A	A
19. The same compressed .	0·7 × 0·05	"	E	A	A
20. The same powdered and compressed .	0·6 × 0·07	"	E	A	A
21. Cylinder of the same .	1·0 × 0·15	"	E	A	A
22. Tourmaline . .	2·1 × 0·1	Paramagnetic	A	E	E
23. A second specimen .	1·1 × 0·1	"	A	E	E
24. A third . . .	0·9 × 0·1	"	A	E	E
25. Sulphate of nickel .	0·9 × 0·3	"	A	E	E
26. A second specimen .	0·6 × 0·2	"	A	E	E
27. Heavy spar . .	0·38 × 0·18	Diamagnetic	E	A	A
28. A second specimen .	0·4 × 0·18	"	E	A	A
29. Carbonate of tin powdered and compressed	0·34 × 0·04	"	E	A	A
30. A second specimen .	length 6 times width	"	E	A	A
31. Ammonio-phosphate of magnesia powdered and compressed .	0·3 × 0·06	"	E	A	A
32. A second specimen .	0·5 × 0·07	"	E	A	A
33. Carbonate of magnesia powdered and compressed . . .	0·45 × 0·04	"	E	A	A
34. Sulphate of magnesia	0·32 × 0·2	"	E	A	A
35. A second specimen .	0·25 × 0·15	"	E	A	A
36. Flour compressed .	0·24 × 0·04	"	E	A	A
37. Oxalate of cobalt .	0·6 × 0·08	Paramagnetic	A	E	E

These experiments might be extended indefinitely, but sufficient are here to enable us to deduce the law of action. In the first place we notice, that all those substances which set equatorial between the points, and axial above and below them, are *diamagnetic*; while all those which set axial between the points, and equatorial above and below them, are *paramagnetic*. When any one of the substances here named is reduced to the spherical form, this rotation is not observed. I possess, for example, four spheres of calcareous spar, and when any one of them is suspended between the points, it takes up a position which is not changed when the sphere is raised or lowered; the crystallographic axis sets equatorial in all positions. A sphere of compressed carbonate of iron, suspended between the points, also sets that diameter along which the pressure is exerted

from pole to pole, and continues to do so when raised or lowered. A sphere of compressed bismuth, on the other hand, sets its line of compression always equatorial. The position taken up by the *spheres* depends upon the *molecular structure* of the substances which compose them; but, when the mass is *elongated*, another action comes into play. Such a mass being suspended with its length horizontal, the *repulsion of its ends* constitutes a mechanical couple which increases in power with the length of the mass; and when the body is long enough, and the local repulsion of the ends strong enough, the couple, when it acts in opposition to the directive tendency due to structure, is able to overcome the latter and to determine the position of the mass.

In all the cases cited, it was so arranged that the length of the body and its structure should act in opposition to each other. Tartaric acid and citric acid cleave with facility in one direction, and, in the specimens used, the planes of cleavage were perpendicular to the length of the body. In virtue of the structure these planes tended to set equatorial, but the repulsion of the elongated mass by the points prevented this, and caused the planes to set axial. When, however, the body was raised or lowered out of the region of local repulsion, and into a position where the distribution of the force was more uniform, the advantage due to length became so far diminished that it was overcome, in turn, by the influence of structure, and the planes of cleavage turned into the equatorial position. In the specimen of saltpetre the shortest horizontal dimension was parallel to the axis of the crystal, which axis, when the influence of form is destroyed, always sets equatorial. A full crystal of calcareous spar will, when the magnetic distribution is tolerably uniform, always set its axis at right angles to the line joining the poles; but the axis is the shortest dimension of the crystal, and, between the points, this mechanical disadvantage compels the influence of structure to succumb to the influence of shape. A cube of calcareous spar, in my possession, may be caused to set the optic axis from pole to pole between the points, but this is evidently due to the elongation of the mass along the diagonals; for, when the corner of the cube succeeds in passing the point of the pole, the mass turns its axis with surprising energy into the equatorial position,

round which it oscillates with great vivacity. Counting the oscillations, I found that eighty-two were performed by the cube, when its axis was equatorial, in the time required to perform fifty-nine, when the axis stood from pole to pole. Heavy spar and cœlestine are beautiful examples of directive action. These crystals, as is well known, can be cloven into prisms with rhombic bases: the principal cleavage is parallel to the base of the prism, while the two subordinate cleavages

Fig. 2.

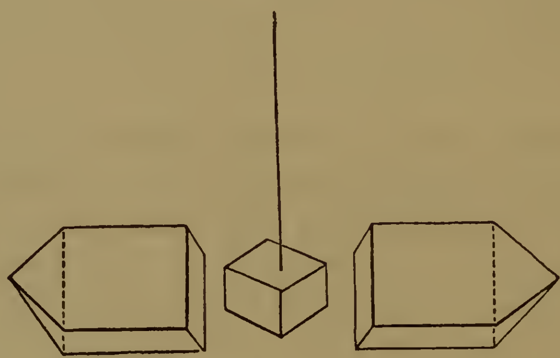


Fig. 3.

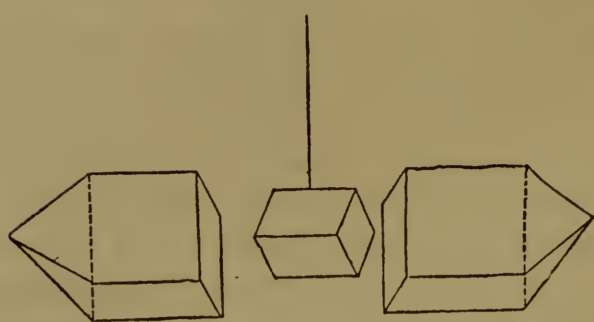


Fig. 4.

constitute the sides. If a short prism be suspended in a tolerably uniform field of force, so that its rhombic ends shall be horizontal, on exciting the magnet the short diagonal will

set equatorial, as shown in fig. 2. If the prism be suspended with its axis and the short diagonal horizontal, the long diagonal being therefore vertical, the short diagonal will retain the equatorial position, while the axis of the prism sets axial as in fig. 3. If the prism be suspended with its long diagonal and axis horizontal, the short diagonal being vertical, and its directive power therefore annulled, the axis will take up the equatorial position, as in fig. 4.

Now as the line which sets equatorial in diamagnetic bodies is that in which the magnetic repulsion acts most strongly,* the crystal before us furnishes a perfect example of a substance possessing three rectangular magnetic axes, no two of which are equal. In the experiment cited in Table II. page 99, the mass was so cut that the short diagonal of the rhombic base was perpendicular to the length of the specimen. Carbonate of tin, and the other powders, were compressed by placing the powder between two clean plates of copper, and squeezing them together in a strong vice. The line of compression in diamagnetic bodies always sets equatorial, when the field of force is uniform, or approximately so; but between points the repulsion of the ends furnishes a couple strong enough to overcome this directive action, causing the longest dimension of the mass to set equatorial, and consequently its line of compression axial.

The antithesis between the deportment of diamagnetic bodies and of paramagnetic ones is thus far perfect. Between the points the former class set equatorial, the latter axial. Raised or lowered, the former set axial, the latter equatorial. The simple substitution of an attractive for a repulsive force produces this difference of effect. A sphere of ferrocyanide of potassium, for example, always sets the line perpendicular to the crystallographic axis from pole to pole; but when we take a full crystal, whose dimension along its axis, as in one of the cases before us, is six times the dimension at right angles to the axis, the attraction of the ends of such a mass is sufficient to overcome the directive action due to structure, and to pull the crystal into the axial position between the points. In a field of uniform force, or between flat poles, the length sets equatorial, and it is the partial attainment of such a field, at a distance from the points, that

* Phil. Mag. S. 4. vol. ii. p. 177.

causes the crystal to turn from axial to equatorial when it is raised or lowered. Beryl is a paramagnetic crystal, and when the influence of form is annulled, it always sets a line perpendicular to the axis of the crystal from pole to pole; a *cube* of this crystal, at present in my possession, shows this deportment whether the poles are pointed or flat: but in the specimen examined the dimension of the crystal along its axis was greatest, and hence the deportment described. It is needless to dwell upon each particular paramagnetic body: the same principle was observed in the preparation and choice of all of them; namely, that the line which, in virtue of the internal structure of the substance, would set axial, was transverse to the length of the body. The directive action due to structure was thus brought into opposition with the tendency of magnetic bodies to set their longest dimension from pole to pole: between the points the latter tendency was triumphant; at a distance, on the contrary, the influence of structure prevailed.

The substance which possesses this directive action in the highest degree is carbonate of iron: when a lozenge, cloven from the crystalline mass, is suspended from the angle at which the crystallographic axis issues, there is great difficulty in causing the plate to set axial. If the points are near, on exciting the magnetism the whole mass springs to one or the other of the points; and when the points are distant, the plate, although its length may be twenty times its thickness, will set strongly equatorial. An excitation by one cell is sufficient to produce this result. In the experiment cited in the table the residual magnetism was found to answer best, as it permitted the ends of the plate to be brought so near to the points that the mass was pulled into the axial position. When the magnet was more strongly excited, and the plate raised so far above the points as to prevent its springing to either of them, it was most interesting to watch the struggle of the two opposing tendencies. Neither the axial nor the equatorial position could be retained; the plate would wrench itself spasmodically from one position into the other, and, like the human spirit operated on by conflicting passions, find rest nowhere.

The conditions which determine the curious effects described in the present chapter may be briefly expressed as follows:—

An elongated diamagnetic body being suspended in the

magnetic field, if the shortest horizontal dimension tend, in virtue of the internal structure of the substance, to set equatorial, it is opposed by the tendency of the longest dimension to take up the same position. Between the pointed poles the influence of length usually predominates; above the points and below them the directive action due to structure prevails.

Hence, the rotation of a diamagnetic body, on being raised or lowered, is always from the equatorial to the axial position.

If the elongated mass be magnetic, and the shortest dimension of the mass tend, in virtue of its structure, to set from pole to pole, it is opposed by the tendency of the longest dimension to take up the same position. Between the points the influence of length is paramount; above and below the points the influence of structure prevails.

Hence, the rotation of magnetic bodies, on being raised or lowered, is always from the axial to the equatorial position.

The error of the explanation which referred many of the above actions to the presence of two conflicting forces, one of which diminished with the distance in a quicker ratio than the other, lies in the supposition, that the assuming of the axial position proved a body to be magnetic, while the assuming of the equatorial position proved a body to be diamagnetic. This assumption was perfectly natural in the early stages of diamagnetic research, when the modification of magnetic force by structure was unknown. Experience however proves that the total mass of a magnetic body continues to be attracted after it has assumed the equatorial position, while the total mass of a diamagnetic body continues to be repelled after it has taken up the axial one.

On the Distribution of the Magnetic Force between two Flat Poles.

In experiments where a uniform distribution of the magnetic force is desirable, flat poles, or magnetised surfaces, have been recommended. It has long been known that the force proceeds with great energy from the edges of such poles: the increase of force from the centre to the edge has been made the subject of a special investigation by M. von Kolke.* The central portion

* Poggendorff's *Annalen*, vol. lxxxi. p. 321.

of the magnetic field, or space between two such magnetised surfaces, has hitherto been regarded as almost perfectly uniform, and indeed for all ordinary experiments the uniformity is sufficient. But, when we examine the field carefully, we find that the uniformity is not perfect. Substituting, for the sake of convenience, the edge of a pole for a point, I studied the phenomena of rotation described in the last section, in a great number of instances, by comparing the deportment of an elongated body, suspended in the centre of the space between two flat poles, with its deportment when suspended between the top or the bottom edges. Having found that the fibre of wood, in masses where form had no influence, always set equatorial, I proposed to set this tendency to contend with an elongation of the mass in a direction at right angles to the fibre. For this purpose, thirty-one little wooden bars were carefully prepared and examined, the length of each bar being about twice its width, and the fibre coinciding with the latter dimension. The bars were suspended from an extremely fine fibre of cocoon silk, and in the centre of the magnetic field each one of them set its length axial, and consequently its fibre equatorial. Between the top and bottom edges, on the contrary, each piece set its longest dimension equatorial, and consequently the fibre axial.

For some time I referred the axial setting of the mass, in the centre of the field, to the directive action of the fibre, though, knowing the extreme feebleness of this directive action, I was surprised to find it able to accomplish what the experiments exhibited. The thought suggested itself, however, of suspending the bars with the fibre vertical, in which position the latter could have no directive influence. Here also, to my surprise, the directive action, though slightly weakened, was the same as before: in the centre of the field the bars took up the axial position. Bars of sulphur, wax, salt of hartshorn, and other diamagnetic substances were next examined: they all acted in the same manner as the wood, and thus showed that the cause of the rotation lay, not in the structure of the substances, but in the distribution of the magnetic force around them. This distribution in fact was such, that the straight line which connected the centre of one pole with that of the opposite one was the line of weakest force. Ohm represents the distribution of electricity upon the surfaces of conductors by regarding the tensions as

ordinates, and erecting them from the points to which they correspond, the steepness of the curve formed by uniting the ends of the ordinates being the measure of the increase or diminution of tension. Taking the centre of the magnetic field as the origin, and drawing horizontal lines axial and equatorial, if we erect the magnetic tensions along these lines, we shall find a steeper curve in the equatorial than in the axial direction. This may be proved by suspending a bit of carbonate of iron in the centre of the magnetic field; on exciting the magnet, the suspended body will not move to the nearest portion of the flat pole, though it may be not more than a quarter of an inch distant, but will move equatorially towards the edges, though they may be two inches distant. The little diamagnetic bars referred to were therefore pushed into the axial position by the force acting with superior power in an equatorial direction.

The results just described are simply due to the recession of the ends of an elongated body from places of stronger to those of weaker force; but it is extremely instructive to observe how this result is modified by structure. If, for example, a plate of bismuth be suspended between the poles with the plane of principal cleavage vertical, the plate will assert the equatorial position from top to bottom; and in the centre with almost the same force as between the edges. The cause of this lies in the structure of the bismuth. Its position in the field depends not so much upon the distribution of the magnetic force around it, as upon the direction of the force *through* it. I will not, however, anticipate matters by entering further upon this subject at present.

Comparative View of Paramagnetic and Diamagnetic Phenomena.

1. *State of Diamagnetic Bodies under Magnetic Influence.*

When a piece of iron is brought near a magnet, it is attracted by the latter: this attraction is not the act of the magnet alone, but results from the mutual action of the magnet and the body upon which it operates. The iron in this case is said to be magnetised by influence; it becomes itself a magnet, and the intensity of its magnetisation varies with the strength of the influencing magnet. Poisson figured the act of magnetisa-

tion as consisting of the decomposition of a neutral magnetic fluid into north and south magnetism, the amount of the decomposition being proportional to the strength of the magnet which produces it. Ampère, discarding the notion of magnetic fluids, figured the molecules of iron as surrounded by currents of electricity, and conceived the act of magnetisation to consist in setting the planes of these molecular currents parallel to each other: the degree of parallelism, or in other words, the intensity of the magnetisation, depending, as in Poisson's hypothesis, upon the strength of the influencing magnet.

The state into which the iron is here supposed to be thrown is a state of constraint, and when the magnet is removed, the substance returns to its normal condition. Poisson's separated fluids rush together once more, and Ampère's molecular currents return to their former irregular positions. As our knowledge increases, we shall probably find both hypotheses inadequate to represent the phenomena; the only thing certain is, that the iron, when acted upon by the magnet, is thrown into an unusual condition, in virtue of which it is attracted; and that the intensity of this condition is a function of the force which produces it.

There are, however, bodies which, unlike iron, offer a great resistance to the imposition of the magnetic state, but when once they are magnetised they do not, on the removal of the magnet, return to their neutral condition, but retain the magnetism impressed on them. It is in virtue of this quality that steel can be formed into compass needles and permanent magnets. This power of resistance and retention is named by Poisson coercive force.

Let us conceive a body already magnetised, and in which coercive force exists in a very high degree—a piece of very hard steel for example—to be brought near a magnet, the strength of which is not sufficient to magnetise the steel further. To simplify the matter, let us fix our attention upon the south pole of the magnet, and conceive it to act upon the north pole of the piece of steel. Let the magnetism of the said south pole, referred to any unit, be M, and of the north pole of the steel, M′; then their mutual attraction, at the unit of distance, is expressed by the product MM′. Conceive now the magnet to increase in power from M to nM, the steel being still supposed

hard enough to resist magnetisation by influence; the mutual attraction now will be

$$nMM',$$

or n times the former attraction; hence when a variable magnetic pole acts on an opposite one of constant power, the attraction is proportional to the strength of the former.

Let us now take a body whose magnetisation varies with that of the magnet: a south pole of the strength M induces in such a body a north pole of the strength M′, and the attraction which results from their mutual action is

$$MM'.$$

Let the strength of the influencing south pole increase from M to nM; then, assuming the magnetism of the body under influence to increase in the same ratio, the strength of the above-mentioned north pole will become nM′, and the attraction, expressed by the product of both, will be

$$n^2MM';$$

that is to say, the attraction of a body magnetised by influence, and whose magnetism varies as the strength of the influencing magnet, is proportional to the *square of the strength* of the latter.

Here then is a mark of distinction between those bodies which have their power of exhibiting magnetic phenomena conferred upon them by the magnet, and those whose actions are dependent upon some constant property of the mass: in the latter case the resultant action will be simply proportional to the strength of the magnet, while in the former case a different law of action will be observed.*

The examination of this point lies at the very foundation of our inquiries into the nature of the diamagnetic force. Is the repulsion of diamagnetic bodies dependent merely on the mass considered as ordinary matter, or is it due to some condition impressed upon the mass by the influencing magnet? This question admits of the most complete answer either by comparing the increase of repulsion with the increase of power in the magnet which produces the repulsion, or by comparing the attraction of a paramagnetic body, which we know to be thrown

* This test was first pointed out in a paper on the Polarity of Bismuth, Phil. Mag. Nov. 1851, p. 333. I have reasons, however, to know that the same thought occurred to M. Poggendorff previous to the publication of my paper.—J. T.

into an unusual condition, with the repulsion of a diamagnetic body, whose condition we would ascertain.

Bars of iron and bismuth, of the same dimensions, were submitted to the action of an electro-magnet, which was caused gradually to increase in power; commencing with an excitation by one cell, and proceeding up to an excitation by ten or fifteen. The strength of the current was in each case accurately measured by a tangent galvanometer. The bismuth bar was suspended between the two flat poles, and, when the magnet was excited, took up the equatorial position. The iron bar, if placed directly between the poles, would, on the excitation of the magnetism, infallibly spring to one of them; hence it was removed to a distance of 2 feet 7 inches from the centre of the space between the poles, and in a direction at right angles to the line which united them. The magnet being excited, the bar, in each case, was drawn a little aside from its position of equilibrium and then liberated, a series of oscillations of very small amplitude followed, and the number of oscillations accomplished in a minute was carefully ascertained. Tables III. and IV. contain the results of experiments made in the manner described with bars of iron and bismuth of the same dimensions.

Table III.

Bar of Iron, No. 1.—Length 0·8 of an inch; width 0·13 of an inch; depth 0·15 of an inch.

Strength of current	Attraction
168	168^2
214	204^2
248	253^2
274	275^2
323	313^2
362	347^2
385	374^2
411	385^2

Table IV.

Bar of Bismuth, No. 1.—Length 0·8 of an inch; width 0·13 of an inch; depth 0·15 of an inch.

Strength of current	Repulsion
78	78^2
136	135^2
184	191^2
226	226^2
259	259^2
287	291^2
341	322^2
377	359^2
411	386^2

These experiments prove, that, up to a strength of about 280, the attractive force operating upon the iron, and the repulsive force acting upon the bismuth, are each proportional to the square of the strength of the magnetising current. For higher powers, both attraction and repulsion increase in a smaller ratio; but it is here sufficient to show that the diamagnetic repulsion follows precisely the same law as the magnetic attraction. So

accurately indeed is this parallelism observed, that while the forces at the top of the tables produce attractions and repulsions exactly equal to the square of the strength of the current, the same strength of 411, at the bottom of both tables, produces in iron an attraction of 385^2, and in bismuth a repulsion of 386^2. The numbers which indicate the strength of current in the first column are the tangents of the deflections observed in each case: neglecting the indices, the figures in the second column express the number of oscillations accomplished in a minute, multiplied by a constant factor to facilitate comparison; the forces operating upon the bars being proportional to the squares of the number of oscillations, the simple addition of the index figure completes the expression of these forces.

In these experiments the bismuth bar set *across* the lines of magnetic force, while the bar of iron set *along* them; the former was so cut from the crystalline mass, that the plane of principal cleavage was parallel to the length of the bar, and in the experiments hung vertical. I thought it interesting to examine the deportment of a bar of bismuth which should occupy the same position, with regard to the lines of force, as the bar of iron; that is to say, which should set its length axial. Such a bar is obtained when the planes of principal cleavage are transverse to the length.

Table V.

Bar of Bismuth, No. 2.—Length 0·8 of an inch; width 0·13 of an inch; depth 0·15 of an inch.

Set axial between the excited poles.

Strength of current	Repulsion
68	67^2
182	187^2
218	218^2
248	249^2
274	273^2
315	309^2
364	350^2
401	366^2

A deportment exactly similar to that exhibited in the foregoing cases is observed here also: up to about 280 the repulsions are exactly proportional to the squares of the current strengths, and from this point forward they increase in a less ratio.

A paramagnetic substance was next examined which set its

length at right angles to the lines of magnetic force: the substance was carbonate of iron. The native crystallised mineral was reduced to powder in a mortar, and the powder was compressed. It was suspended, like the bismuth, between the flat poles, with its line of compression horizontal. When the poles were excited, the compressed bar set the line of pressure from pole to pole, and consequently its length equatorial.

Table VI.

Bar of compressed Carbonate of Iron.—Length 0·95 of an inch; width 0·17 of an inch; depth 0·23 of an inch.

Set equatorial between the excited poles.

Strength of current	Attraction
74	74^2
135	133^2
179	180^2
214	218^2
249	248^2
277	280^2
341	330^2
381	353^2

It is needless to remark upon the perfect similarity of deportment here exhibited to the cases previously recorded.

In experiments made with bars of sulphate of iron the same law of increase was observed.

These experiments can leave little doubt upon the mind, that if a magnetic body be attracted in virtue of its being converted into a magnet, a diamagnetic body is repelled *in virtue of its being converted into a diamagnet.* On no other assumption can it be explained, why the repulsion of the diamagnetic body, like the attraction of the magnetic one, increases in a so much quicker ratio than the force of the magnet which produces the repulsion. But, as this is a point of great importance, I will here introduce corroborative evidence, derived from modes of experiment totally different from the method already described. By a series of measurements with the torsion balance, in which the attractive and repulsive forces were determined directly, with the utmost care, the relation of the strength of the magnet to the force acting upon the following substances was found to be as follows:—

Table VII.

Spheres of Native Sulphur.

Strength of magnet	Ratio of repulsions
96	95^2
153	158^2
222	224^2
265	264^2
316	316^2

Table VIII.

Spheres of Carbonate of Lime.

Strength of magnet	Ratio of repulsions
134	134^2
172	173^2
213	212^2
259	264^2
310	311^2

Table IX.

Spheres of Carbonate of Iron.

Strength of magnet	Ratio of attractions
66	66^2
89	89^2
114	114^2
141	141^2

These results confirm those of M. E. Becquerel,* whose experiments first showed that the repulsion of diamagnetic bodies follows the same law as the attraction of magnetic ones.

Bar of Sulphur.—Length 25 millims.; weight 840 milligrms.

Squares of the magnetic intensities	Quotients of the repulsions by the magnetic intensities
36·58	0·902
27·60	0·929
26·84	0·906
16·33	0·920

The constancy of the quotient in the second column proves that the ratio of the repulsions to the squares of the magnetic intensities is a ratio of equality.

I will also cite a series of experiments by Mr. Joule,† which that excellent philosopher adduces in confirmation of the results obtained by M. E. Becquerel and myself.

Bar of Bismuth.

Strength of magnet	Repulsions
1	1^2
2	2^2
4	4^2

Let us contrast these with the results obtained by Mr. Joule, on permitting the magnet to act upon a hard magnetic needle.

* *Annales de Chimie et de Physique*, 3rd series, vol. xxviii. p. 302.

† Phil. Mag., 4th series, vol. iii. p. 32.

Magnetic Needle.—Length 1·5 of an inch.

Strength of magnet	Attraction
1	1
2	2
4	4

Here we find experiment in strict accordance with the theoretical deduction stated at the commencement of the present chapter. The intensity of the magnetism of the steel needle is constant, for the steel resists magnetisation by influence; the consequence is that the attraction is simply proportional to the strength of the magnet.

A consideration of the evidence thus adduced from independent sources, and obtained by different methods, must, I imagine, render the conclusion certain that diamagnetic bodies, like magnetic ones, exhibit their phenomena in virtue of a state of magnetisation induced in them by the influencing magnet. This conclusion is in no way invalidated by the recent researches of M. Plücker, on the law of induction in paramagnetic and diamagnetic bodies, but, on the contrary, derives support from his experiments. With current strengths which stand in the ratio of 1 : 2, M. Plücker finds the repulsion of bismuth to be as 1 : 3·62, which, though it falls short of the ratio of 1 : 4, as the law of increase according to the square of the current would have it, suffices to show that the bismuth was not passive, but acted the part of an induced diamagnet in the experiments. In the case of the iron itself, M. Plücker finds a far greater divergence; for here currents which stand in the ratio of 1 : 2 produce attractions only in the ratio of 1 : 2·76.

2. *Duality of Diamagnetic Excitement.*

Having thus safely established the fact that diamagnetic bodies are repelled, in virtue of a certain state into which they are cast by the influencing magnet, the next step of our inquiry is;—Will the state evoked by one magnetic pole facilitate, or prevent, the repulsion of the diamagnetic body by a second pole of an opposite quality? If the force of repulsion were an action on the mass, considered as ordinary matter, this mass, being repelled by both the north and the south pole of a magnet, when they operate upon it separately, ought to be repelled by the sum of the forces of the two poles where they act upon it together.

But if the excitation of diamagnetic bodies be of a *dual* nature, as is the case with the magnetic bodies, then it may be expected that the state excited by one pole will not facilitate, but on the contrary prevent, the repulsion of the mass by a second opposite pole.

To solve this question the apparatus sketched in fig. 5*a*, Plate II. was made use of. AB and CD are two helices of copper wire 12 inches long, of 2 inches internal, and of $5\frac{1}{2}$ inches external diameter. Into them fit soft iron cores 2 inches thick: the cores are bent as in the figure, and reduced to flat surfaces along the line *ef*, so that when the two semicylindrical ends are placed together, they constitute a cylinder of the same diameter as the cores within the helices.* In front of these poles a bar of pure bismuth *gh* was suspended by cocoon silk; by imparting a little torsion to the fibre, the end of the bar was caused to press gently against a plate of glass *ik*, which stood between it and the magnets. By means of a current reverser the polarity of one of the cores could be changed at pleasure; thus it was in the experimenter's power to excite the cores, so that the poles PP′ should be of the same quality, or of opposite qualities.

The bar, being held in contact with the glass by a very feeble torsion, a current was sent round the cores, so that they presented two poles of the same name to the suspended bismuth; the latter was promptly repelled, and receded to the position dotted in the figure. On interrupting the current it returned to the glass as before. The cores were next excited, so that two poles of opposite qualities acted upon the bismuth; the latter remained perfectly unmoved.†

This experiment shows that the state, whatever it may be, into which bismuth is cast by one pole, so far from being favourable to the action of the opposite pole, completely neutralises the effect of the latter. A perfect analogy is thus established between the deportment of the bismuth and that of iron under the same circumstances; for it is well known that a similar neutralisation occurs in the latter case. If the repulsion depended upon the *strength* of the poles, without reference to

* The ends of the semicylinders were turned so as to present the blunted apex of a cone to the mass of bismuth.

† A shorter bar of bismuth than that here sketched, with a light index attached to it, makes the repulsion more evident. It may be thus rendered visible throughout a large lecture-room.

their *quality*, the repulsion, when the poles are of opposite names, ought to be *greater* than when they are alike; for in the former case the poles are greatly strengthened by their mutual inductive action, while, in the latter case, they are enfeebled by the same cause. But the fact of the repulsion being dependent on the quality of the pole, demonstrates that the substance is capable of assuming a condition peculiar to each pole, or in other words, is capable of a *dual* excitation.* The experiments from which these conclusions are drawn are a manifest corroboration of those made by M. Reich with steel magnets.

If we suppose the flat surfaces of the two semicylinders which constitute the ends of the cores to be in contact, and the cares so excited that the poles P and P′ are of different qualities, the arrangement it is evident, forms a true electro-magnet of the horseshoe form; and here the pertinency of a remark made by M. Poggendorff, with his usual clearness of perception, becomes manifest; namely, that if the repulsion of diamagnetic bodies be an indifferent one of the mass merely, there is no reason why they should not be repelled by the centre of a magnet, as well as by its ends.

3. *Separate and joint action of a Magnet and a Voltaic Current on Paramagnetic and Diamagnetic Bodies.*

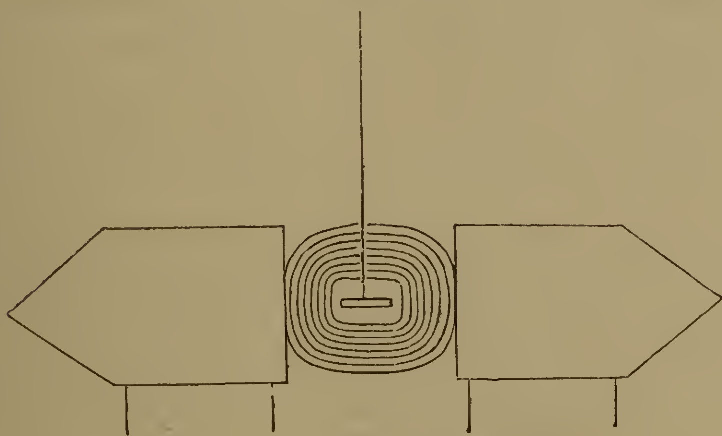
In operating upon bars of bismuth with the magnet, or the current, or both combined, it was soon found that the gravest mistakes might be committed if the question of molecular structure was not attended to; that it is not more indefinite to speak of the volume of a gas without giving its temperature, than to speak of the deportment of bismuth without stating the relation of the form of the mass to the planes of crystallisation. Cut in one direction, a bar of bismuth will set its length parallel to an electric current passing near it; cut in another

* Since the above was written, the opinion has been expressed to me, that the action of the *unlike* poles, in the experiment before us, is 'diverted' from the bismuth upon each other, the absence of repulsion being due to this diversion, and not to the neutralisation of inductions in the mass of the bismuth itself. Many, however, will be influenced by the argument as stated in the text, who would not accept the interpretation referred to in this note; I therefore let the argument stand, and hope at no distant day to return to the subject.—J. T., May 5, 1855.

direction, it will set its length perpendicular to the same current. It was necessary to study the deportment of both of these bars separately.

A helix was formed of covered copper wire one-twentieth of an inch thick: the space within the helix was rectangular, and

Fig. 6.



was 1 inch long, 0·7 inch high, and 1 inch wide: the external diameter of the helix was 3 inches. Within the rectangular space the body to be examined was suspended by a fibre which descended through a slit in the helix. The latter was placed between the two flat poles of an electro-magnet, and could thus be caused to act upon the bar within it, either alone or in combination with the magnet. The disposition will be at once understood from fig. 6, which gives a front view of the arrangement.

a.—*Action of Magnet alone: Division of bars into Normal and Abnormal.*

A bar of soft iron suspended in the magnetic field will set its longest dimension from pole to pole: this is the normal deportment of paramagnetic bodies. A bar of bismuth, whose planes of principal cleavage are throughout parallel to its length, suspended in the magnetic field with the said planes vertical, will set its longest dimension at right angles to the line joining the poles: this is the normal deportment of diamagnetic bodies. We will, therefore, for the sake of distinction, call the former a *normal paramagnetic bar*, and the latter a *normal diamagnetic bar*.

A bar of compressed carbonate of iron dust, whose shortest dimension coincides with the line of pressure, will, when suspended in the magnetic field with the said line horizontal, set its length equatorial. A bar of compressed bismuth dust, similarly suspended, or a bar of bismuth whose principal planes of crystallisation are transverse to its length, will set its length axial in the magnetic field. We will call the former of these an *abnormal paramagnetic bar*, and the latter an *abnormal diamagnetic bar*.

b.—*Action of Current alone on normal and abnormal bars.*

A normal paramagnetic bar was suspended in the helix above described; when a current was sent through the latter, the bar set its longest horizontal dimension parallel to the axis of the helix, and consequently perpendicular to the coils.

An *abnormal paramagnetic bar* was suspended in the same manner; when a current was sent through the helix, the bar set its longest dimension perpendicular to the axis of the helix, and consequently parallel to the coils.

A *normal diamagnetic bar* was delicately suspended in the same helix; on the passage of the current it acted precisely as the abnormal magnetic bar; setting its longest dimension perpendicular to the axis of the helix and parallel to the coils. When a fine fibre and sufficient power are made use of, this deportment is obtained without difficulty.

An *abnormal diamagnetic bar* was suspended as above; on the passage of the current it acted precisely as the normal magnetic bar: it set its length parallel to the axis of the helix and perpendicular to the coils. Here also, by fine manipulation, the result is obtained with ease and certainty.

c.—*Action of Magnet and Current combined.*

In examining this subject, eight experiments were made with each particular bar; it will be remembered that fig. 6 gives a general view of the arrangement.

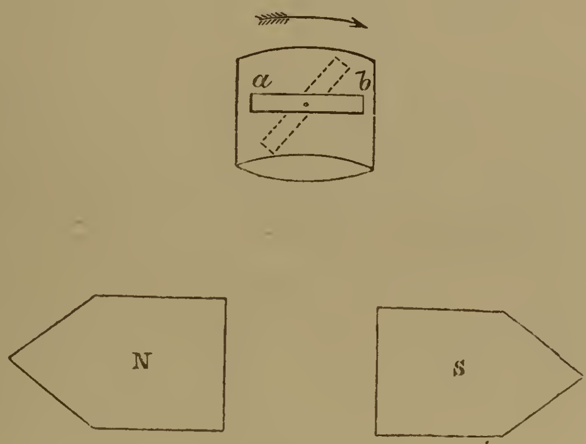
1. Four experiments were made in which the *magnet* was excited first, and after the suspended bar had taken up its position of equilibrium, the deflection produced by the passage of a current through the surrounding helix was observed.

2. Four experiments were made in which the *helix* was excited first, and when the bar within it had taken up its position of equilibrium, the magnetism was developed and the consequent deflection observed.

Normal Paramagnetic Bar.

In experimenting with iron it was necessary to place it at some distance from the magnet, otherwise the attraction of the entire mass by one or the other pole would completely mask the action sought. Fig. 7 represents the disposition of things

Fig. 7.



in these experiments: N and S indicate the north and south poles of the magnet; *ab* is the bar of iron; the helix within which the bar was suspended is shown in outline around it; the arrow shows the direction of the current in the *upper half* of the helix; its direction in the under portion would, of course, be the reverse.

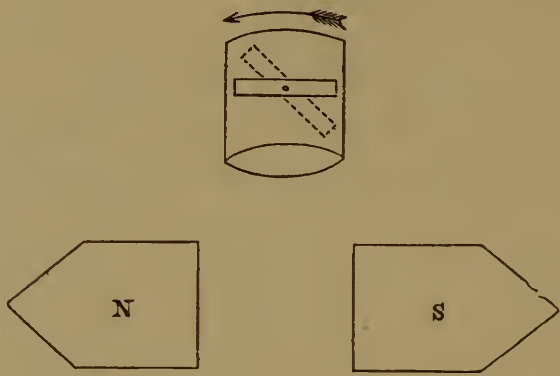
On exciting the magnet, the bar of iron set itself parallel to the line joining the poles, as shown by the unbroken line in fig. 7.

When the direction of the current in the helix was that indicated by the arrow, the bar was deflected towards the position dotted in the figure.

Interrupting the current in the helix, and permitting the magnet to remain excited, the bar returned to its former position: the current was now sent through the helix in the direction of the arrow, fig. 8; the consequent deflection was towards the dotted position.

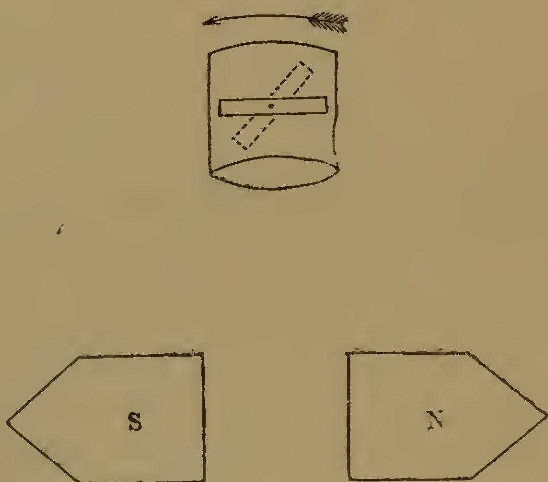
Both the current which excited the magnet and that which passed through the helix were now interrupted, and the

Fig. 8.



polarity of the magnet was reversed. On sending a current through the helix in the direction of the arrow, the deflection of the bar was from the position of the defined line to that of the dotted one, fig. 9.

Fig. 9.

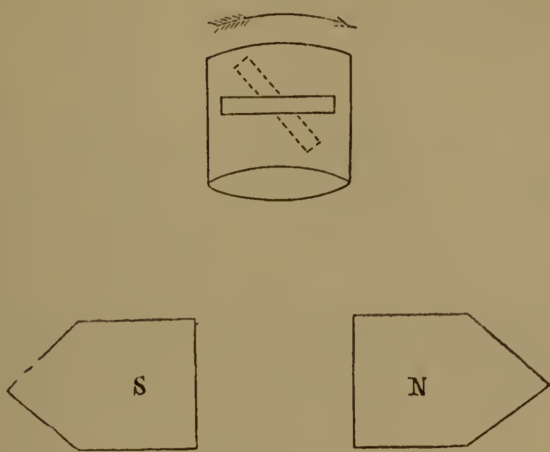


Interrupting the current through the helix, and permitting the bar to come to rest under the influence of the magnet alone, a current was sent through the helix in a direction opposed to its former one: the deflection produced was that shown in fig. 10.

The position of equilibrium finally assumed by the bar depends, of course, upon the ratio of the forces acting upon it: in these experiments, the bar, in its final position, enclosed an angle of about 50 degrees with the axial line.

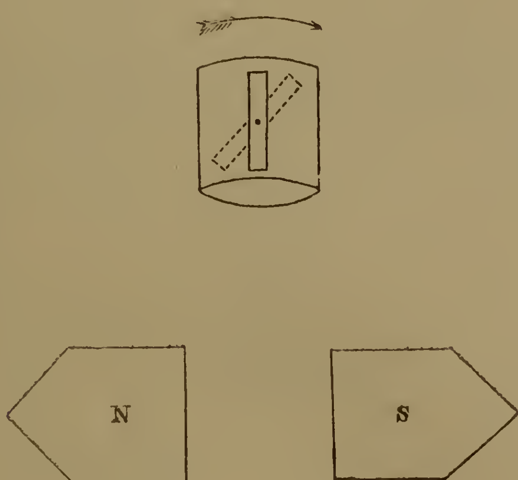
A series of experiments was next made, in which the bar was first acted on by the current passing through the helix,

Fig. 10.



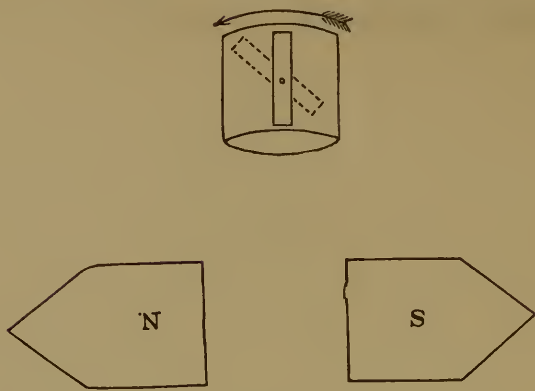
the magnet being brought to bear upon it afterwards. On the passage of the current through the helix in the direction shown in fig. 11, the bar set its length parallel to the axis of the latter. On exciting the magnet so that its polarity was that indicated by the letters N and S in the figure, the deflection was towards the dotted position.

Fig. 11.



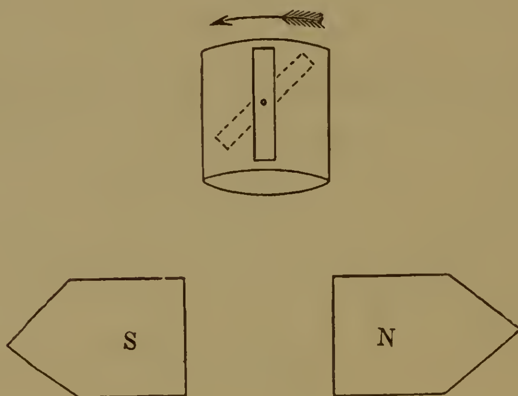
Interrupting the current through both magnet and helix, and reversing the current through the latter, the bar came to rest, as before, parallel to the axis: on exciting the magnet, as in the last case, the deflection was that shown in fig. 12.

Fig. 12.



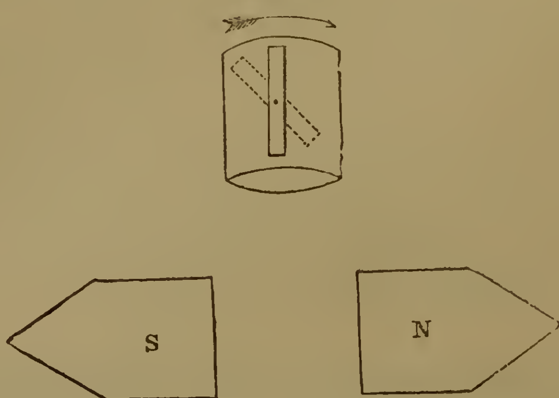
Preserving the same current in the helix, and reversing the polarity of the magnet, the deflection was that shown in fig. 13.

Fig. 13.



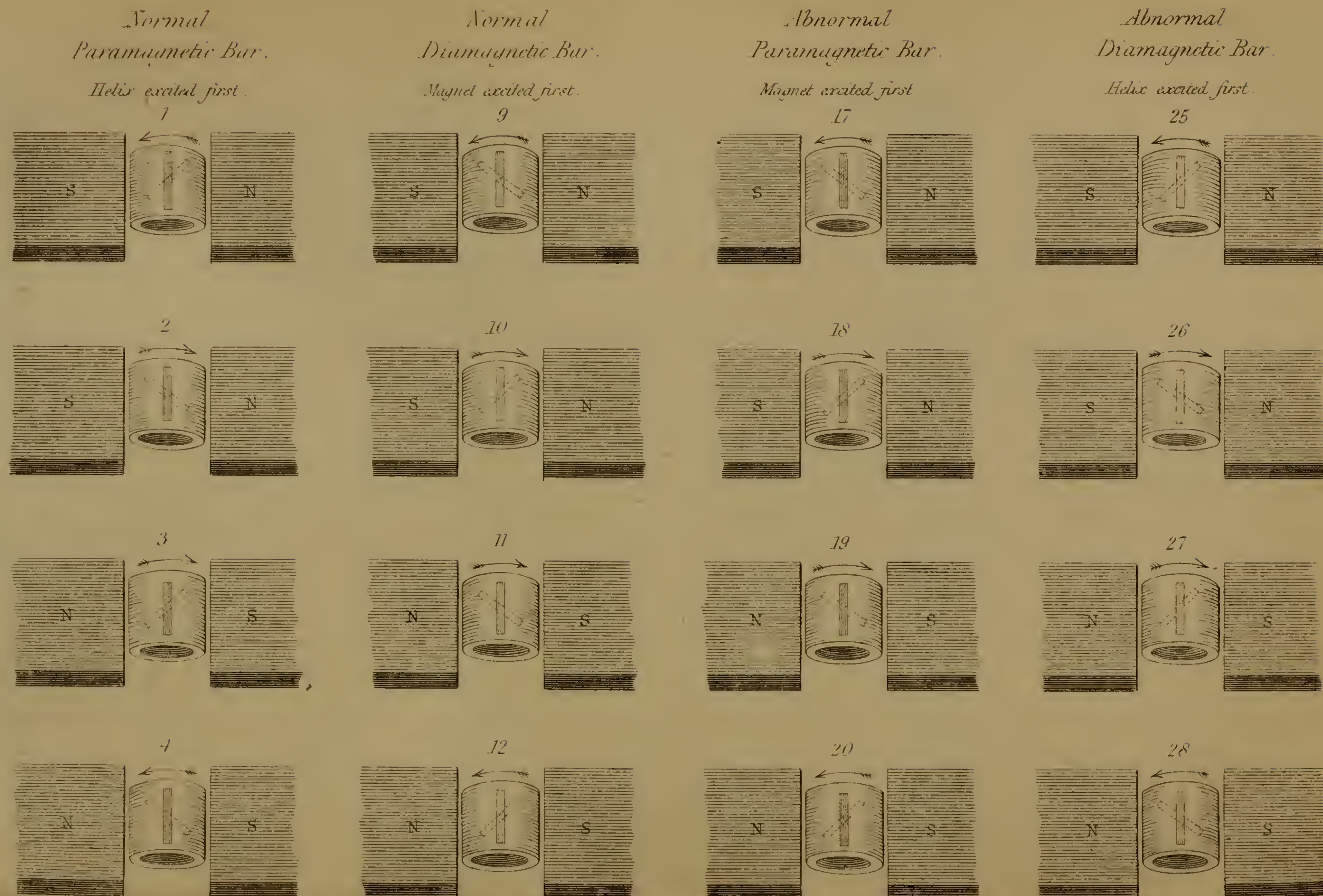
Preserving the magnet poles as in the last experiment, and reversing the current in the helix, the deflection was that shown in fig. 14.

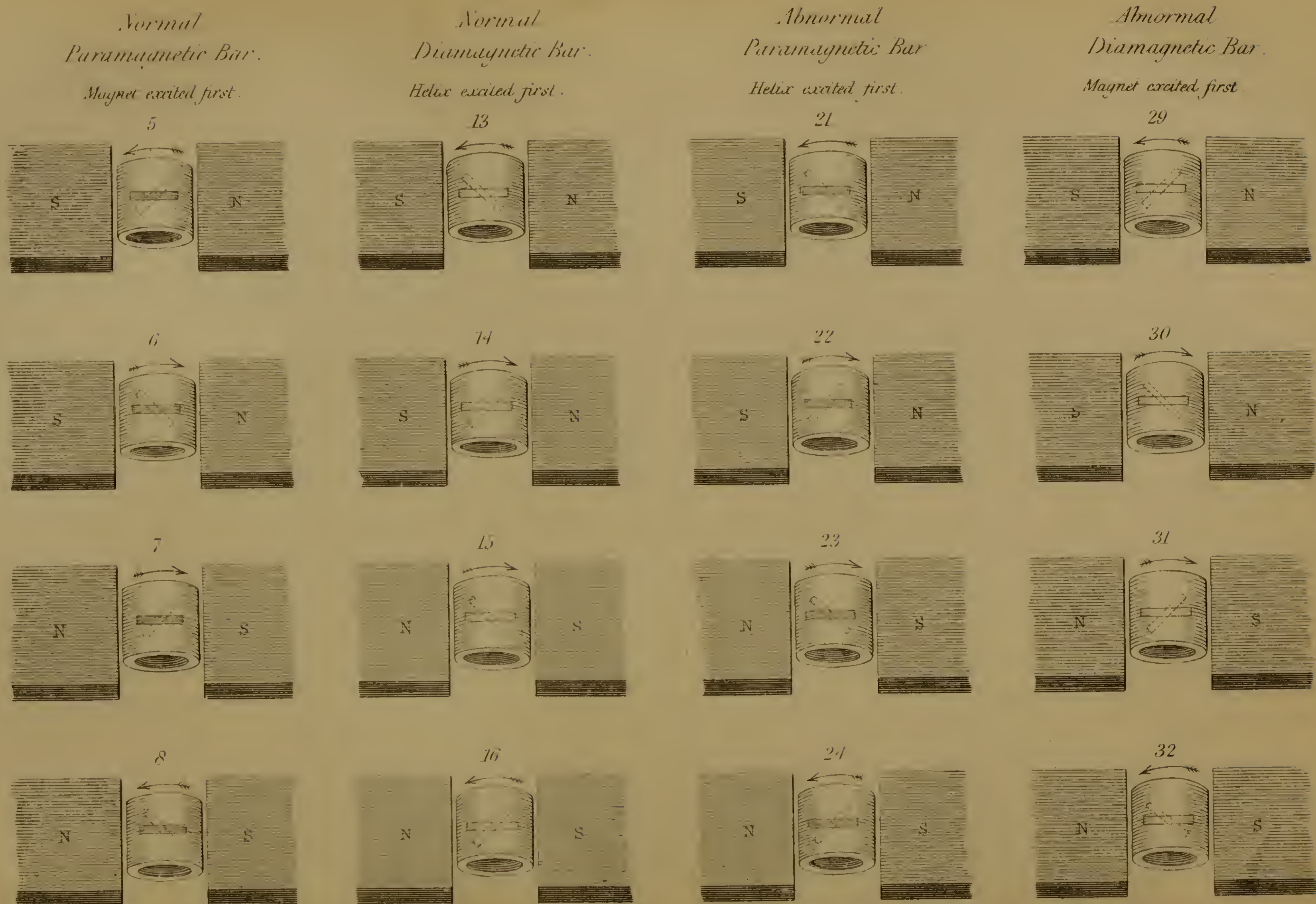
Fig. 14.



In these cases, the bar, in its final position of equilibrium, enclosed an angle of about 40 degrees with the axial line.

Plate 1


Normal
Paramagnetic Bar.
Helix excited first.
Normal
Diamagnetic Bar.
Magnet excited first.
Abnormal
Paramagnetic Bar.
Magnet excited first
Abnormal
Diamagnetic Bar.
Helix excited first.
1
9
17
25
2
10
18
26
3
11
19
27
4
12
20
28
S
N


Normal
Paramagnetic Bar.
Magnet excited first.
Normal
Diamagnetic Bar.
Helix excited first.
Abnormal
Paramagnetic Bar
Helix excited first.
Abnormal
Diamagnetic Bar.
Magnet excited first.
5
S
N
13
S
N
21
S
N
29
S
N
6
S
N
14
S
N
22
S
N
30
S
N
7
N
S
15
N
S
23
N
S
31
N
S
8
N
S
16
N
S
24
N
S
32
N
S

Normal Diamagnetic Bar.

The above experiments exhibit to us the deportment of the normal paramagnetic body under a great variety of conditions, and our next step is to compare with it the deportment of the normal diamagnetic body under the same circumstances.

For the sake of increasing the force, the helix was removed from its lateral position and placed between the two poles, as in fig. 6, p. 117. The normal diamagnetic bar was suspended within the helix and submitted to the self-same mode of examination as that applied in the case of the paramagnetic body.

The polarity first excited was that shown by the letters S and N (south and north) in fig. 9, Plate I., and the position of rest, when the magnet alone acted, was at right angles to the line joining the poles, as shown in unbroken outline; on sending a current through the helix in the direction of the arrow, the deflection was towards the position dotted out.

Preserving the magnetic polarity as in the last experiment, the direction of the current through the helix was reversed, and the deflection was that shown in fig. 10, Plate I. [In all cases the motion is to be regarded as taking place *from* the position shown by the full line *to* that shown by the dotted line.]

Reversing the polarity of the magnet, and sending the current through the helix in the direction of the last experiment, the deflection was that shown in fig. 11.

Preserving the last magnetic poles, and sending the current through the helix in the opposite direction, the deflection was that shown in fig. 12.

In the following four experiments the helix was excited first.

Operated upon by the helix alone, the suspended bar set its length parallel to the convolutions, and perpendicular to the axis of the coil, as shown by the unbroken outline: the direction of the current was first that shown in fig. 13, Plate I*a*. When the magnet was excited, the bar was deflected towards the dotted position.

Interrupting both currents, then, and permitting the bar to come to rest, reversing the current in the helix, the bar set as before parallel to the coils. When the magnet was excited, as in the last experiment, the deflection was that shown in fig. 14.

Preserving the helix current as in the last experiment, when the polarity of the magnet was reversed, the deflection was that shown in fig. 15.

Interrupting both, and reversing the current in the helix; when the magnet was excited as in the last experiment, the deflection was that shown in fig. 16.

In a paper on the 'Polarity of Bismuth,' published in the 'Philosophical Magazine,' ser. 4. vol. ii., and in Poggendorff's *Annalen*, vol. lxxxvii., an experiment is recorded showing the deportment exhibited by fig. 11, Plate I. of the present series. In a recent memoir on the same subject, M. v. Feilitzsch* states that he has sought this result in vain. Sometimes he observed the deflection at the moment of closing the circuit, but conceived that it must be ascribed to the action of induced currents; for immediately afterwards a deflection in the opposite direction was observed, which deflection proved to be the permanent one.

I have repeated the experiment here referred to with all possible care; and the result is that described in the remarks which refer to fig. 11. This result agrees in all respects with that described in my former paper. With a view to quantitative measurement, a small graduated circle was constructed and placed underneath the bar of bismuth suspended within the helix. The effect, as will be seen, is not one regarding which a mistake could be made on account of its minuteness: operating delicately, and choosing a suitable relation between the strength of the magnet and that of the spiral,† on sending a current through the latter as in fig. 11, the bismuth bar was deflected so forcibly that the limit of its first impulsion reached 120° on the graduated circle underneath. [An action entirely due to the extreme caution bestowed upon the experiment, in which *power* and *delicacy* were combined.] The permanent deflection of the bar amounted to 60° in the same direction, and hence the deportment could in no wise be ascribed to induced currents, which vanish immediately. Before sending the current through the helix, the bar was acted on by the magnet alone, and pointed to zero.

* Poggendorff's *Annalen*, vol. xcii. p. 395.

† In most of these experiments the spiral was excited by ten cells, the magnet by two.

Though it was not likely that the shape of the poles could have any influence here, I repeated the experiment, using the hemispherical ends of two soft iron cores as poles: the result was the same.

A pair of poles with the right and left-hand edges rounded off, showed the same deportment.

A pair of poles presenting chisel edges to the helix showed the same deportment.

Various other poles were made use of, some of which appeared to correspond exactly with those figured by M. v. Feilitzsch; but no deviation from the described deportment was observed. To test the polarity of the magnet, a magnetic needle was always at hand: once or twice the polarity of the needle became reversed, which, had it not been noticed in time, would have introduced confusion into the experiments. Here is a source of error against which, however, M. v. Feilitzsch has probably guarded himself. Some irregularity of crystalline structure may also have influenced the result. With 'chemically pure zinc' M. v. Feilitzsch obtained the same deflection that I obtained with bismuth: now chemically pure zinc is *diamagnetic*,* and hence its deportment is corroborative of that which I have observed. M. v. Feilitzsch, however, appears to regard the zinc used by him as magnetic; but if this be the case, it cannot have been chemically pure. It is necessary to remark that I have called the north pole of the electro-magnet that which attracts the south, or unmarked end of a magnetic needle; and I believe this is the custom throughout Germany.

Abnormal Paramagnetic Bar.

This bar consisted of compressed carbonate of iron dust, and was suspended within the helix with the line of compression, which was its shortest dimension, horizontal. As in the cases already described, it was first acted upon by the magnet alone; having attained its position of equilibrium, a current was sent through the helix, and the subsequent deflection was observed.

The magnet being excited as shown by the letters S and N in fig. 17, Plate I., the bar set its length equatorial; on sending a current through the helix in the direction of the arrow, the bar was deflected to the dotted position.

* Phil. Mag. vol. xxviii. p. 456.

Reversing the current in the helix, but permitting the magnet to remain as before, the deflection was that shown in fig. 18.

Interrupting all, and reversing the polarity of the magnet; on sending the current through as in the last case, the deflection was that shown in fig. 19.

Reversing the current, but preserving the last condition of the magnet, the deflection was that shown in fig. 20.

In the subsequent four experiments the helix was excited first.

It is to be remembered that whatever might be the direction of the current through the helix, the bar always set its length perpendicular to the axis of the latter, and parallel to the coils.

When the direction of the helix current, and the polarity of the magnet, were those shown in fig. 21, Plate I*a*., the deflection was to the dotted position.

Interrupting all, and reversing the current in the helix; on exciting the magnet the deflection was that shown in fig. 22.

Changing the polarity of the magnet, and preserving the helix current in its former direction, the deflection was that shown in fig. 23.

Interrupting all, and reversing the current through the helix; when the magnetism was developed the deflection was that shown in fig. 24.

Abnormal Diamagnetic Bar.

This bar consisted of a prism of bismuth whose principal planes of crystallisation were perpendicular to its length: the mode of experiment was the same as that applied in the other cases.

Acted upon by the magnet alone, the bar set its length from pole to pole: the magnetic excitation being that denoted by fig. 29, Plate I*a*., a current was sent through the helix in the direction of the arrow; the bar was deflected to the dotted position.

Reversing the current through the helix, the deflection was that shown in fig. 30.

Interrupting both currents and reversing the magnetic poles; on sending a current through the helix as in the last experiment, the deflection was that shown in fig. 31.

Reversing the current through the helix, the deflection was that shown in fig. 32.

In the subsequent four experiments the helix was excited first.

Sending a current through the helix in the direction denoted by the arrow, the bar set its length at right angles to the convolutions, and parallel to the axis of the helix; when the magnetism was excited as in fig. 25, Plate I., the deflection was to the dotted position.

When the current was sent through the helix in an opposite direction, the deflection was that shown in fig. 26.

Interrupting both currents, and reversing the poles of the magnet; on sending a current through the helix as in the last experiment, the deflection was that shown in fig. 27.

Reversing the current in the helix, the deflection was that shown in fig. 28.

In all these cases the position of equilibrium due to the first force was attained, before the second force was permitted to act.

It will be observed, on comparing the deportment of the normal paramagnetic bar with that of the normal diamagnetic one, that the position of equilibrium taken up by the latter, when operated on by the helix alone, is the same as that taken up by the former when acted on by the magnet alone: in both cases the position is from pole to pole of the magnet. A similar remark applies to the abnormal para- and diamagnetic bars. It will render the distinction between the deportment of both classes of bodies more evident, if the position of the two bars, before the application of the second force, be rendered one and the same. When both the bars, acted on by one of the forces, are axial, or both equatorial, the contrast or coincidence, as the case may be, of the deflections from this common position by the second force will be strikingly manifest.

To effect the comparison in the manner here indicated, the figures have been collected together and arranged upon Plate I. The first column represents the deportment of the normal paramagnetic bar under all the conditions described; the second column, that of the normal diamagnetic bar; the third shows the deportment of the abnormal paramagnetic bar, and the fourth that of the abnormal diamagnetic bar.

A comparison of the first two columns shows us that the deportment of the normal magnetic bar is perfectly antithetical to

that of the normal diamagnetic one. When, on the application of the second force, an end of the former is deflected to the right, the same end of the latter is deflected to the left. When the position of equilibrium of the magnetic bar, under the joint action of the two forces, is from N.E. to S.W., then the position of equilibrium for the diamagnetic bar is invariably from N.W. to S.E. There is no exception to this antithesis, and I have been thus careful to vary the conditions of experiment in all possible ways, on account of the divergent results obtained by other inquirers. In his recent memoirs upon this subject M. v. Feilitzsch states that he has found the deflection of diamagnetic bodies, under the circumstances here described, to be precisely the same as that of paramagnetic bodies: this result is of course opposed to mine; but when it is remembered that the learned German worked confessedly with the 'roughest apparatus,' and possessed no means of eliminating the effects of structure, there seems little difficulty in referring the discrepancy between us to its proper cause.

The same perfect antithesis will be observed in the case of the abnormal bars, on a comparison of the third and fourth columns. In all cases then, whether we apply the magnet singly, or the current singly, or the magnet and current combined, the deportment of the normal diamagnetic bar is opposed to that of the normal paramagnetic one, and the deportment of the abnormal paramagnetic bar is opposed to that of the abnormal diamagnetic one. But if we compare the normal paramagnetic with the abnormal diamagnetic bar, we see that the deportment of the one is identical with that of the other.* The same identity of action is observed when the normal diamagnetic bar is compared with the abnormal paramagnetic one. The necessity of taking molecular structure into account in experiments of this nature could not, I think, be more strikingly exhibited.

For each of the bars, under the operation of the two forces,

* Identical to the eye, but not to the mind. The notion appears to be entertained by some, that, by changing molecular structure, I had actually converted paramagnetic substances into diamagnetic ones, and *vice versâ.* No such change, however, can cause *the mass* of a diamagnetic body suspended by its centre of gravity to be *attracted*, or the mass of a paramagnetic body to be *repelled.* But by a change of molecular structure, one of the forces may be so caused to apply itself that it shall present to the eye all the *directive* phenomena exhibited by the other.—J. T., May 5, 1855.

there is an oblique position of equilibrium: on the application of the second force, the bar swings like a pendulum beyond this position, oscillates round it, and finally comes to rest there. Hence, if before the application of the second force the bar occupy the axial position, the deflection, when the second force is applied, appears to be from the axis to the equator; but if it first occupy the equatorial position, the deflection appears to be from the equator to the axis.

It has been already shown that the repulsion of diamagnetic bodies is to be referred to a state of excitement induced by the magnet, and it has been long known that the attraction of paramagnetic bodies is due to the same cause. The experiments just described exhibit to us bars of both classes of bodies moving in the magnetic field: such motions occur in virtue of the induced state of the body, and the relation of that state to the forces which act upon the mass. We have seen that in all cases the antithesis between both classes of bodies is maintained. Whatever, therefore, the state of the paramagnetic bar under magnetic excitement may be, a precisely antithetical state would produce all the phenomena of the diamagnetic bar. If the bar of iron be polar, a reverse polarity on the part of bismuth would produce the effects observed. From this point of view all the movements of diamagnetic bodies become perfectly intelligible, and the experiments to be recorded in the next chapter are not calculated to diminish the probability of the conclusion that diamagnetic bodies possess a polarity opposed to that of magnetic ones.

The phenomena to which we have thus far referred consist in the rotations of elongated bars about their axes of suspension. The same antithesis, however, presents itself when we compare the *motion of translation* of a paramagnetic body, within the coil, with that of a diamagnetic one. A paramagnetic sphere was attached to the end of a horizontal beam and introduced into the coil: the magnet being excited, the sphere could be made to traverse the space within the coil in various directions, by properly varying the current through the coil. A diamagnetic sphere was submitted to the same examination, and it was found that the motions of both spheres, when operated on by the same forces, were always in opposite directions.

V. Further Comparison of Paramagnetic and Diamagnetic Phenomena:—Diamagnetic Polarity.

When an iron bar is placed within a helix, it is well known that on sending a current through the latter the bar is converted into a magnet, one end of the bar thus excited being attracted, and the other end repelled by the same magnetic pole. In this *twoness* of action consists what is called the *polarity* of the bar: we will now consider whether a bar of bismuth exhibits similar effects.

Fig. 39, Plate II. represents the disposition of the apparatus used in the examination of this question. AB is a helix of covered copper wire one-fifteenth of an inch in thickness: the length of the helix is 5 inches, external diameter 5 inches, and internal diameter 1·5 inch. Within this helix a cylinder of bismuth $6\frac{1}{2}$ inches long and 0·4 of an inch in diameter was suspended. The suspension was effected by means of a light beam, from two points of which, sufficiently distant from each other, depended two silver wires each ending in a loop: into these loops, $l l'$, the bar of bismuth was introduced, and the whole was suspended by a number of fibres of unspun silk from a suitable point of support. Fig. 39*a* is a side view of the arrangement used for the suspension of the bar. Before introducing the latter within the helix, it was first suspended in a receiver, which protected it from air currents, and in which it remained until the torsion of the fibre had exhausted itself: the bar was then removed, and the beam, without permitting the fibre to twist again, was placed over the helix so as to receive the bar introduced through the latter. From the ends of this helix two wires passed to a current reverser R, from which they proceeded further to the poles of a voltaic battery. CD and EF are two electro-magnetic helices, each 12 inches long, $5\frac{1}{2}$ inches external and 2 inches internal diameter. The wire composing them is one-tenth of an inch thick, and so coiled that the current could be sent through four wires simultaneously. Within these helices were introduced two cores of soft iron 2 inches thick and 14 inches long: the ends of the cores appear at P and P′. The helices were so connected together that the same current excited both, thus developing the same magnetic strength in the poles P P′. From the ends of the helices wires

Fig. 5.a

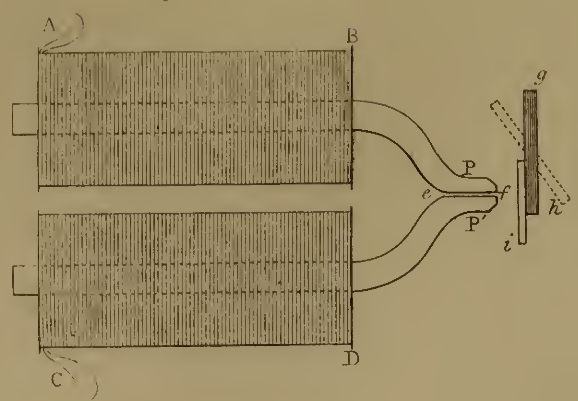
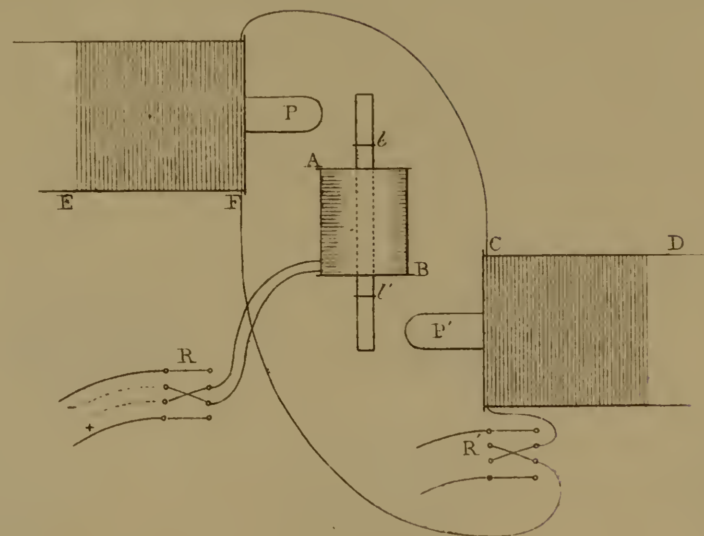
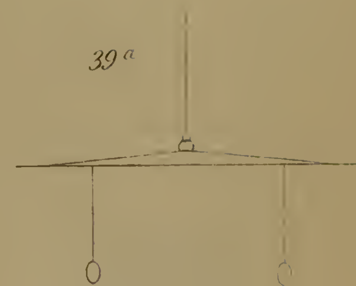


Fig. 39.

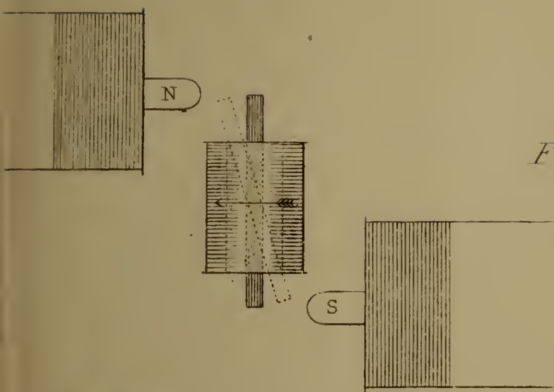


39 a



Bar of Iron — *Bar of Bismuth.*

Fig. 40.



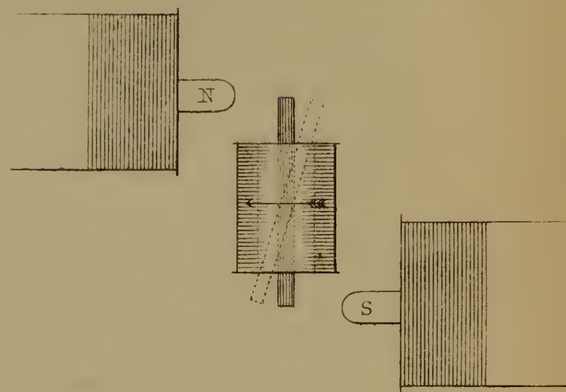
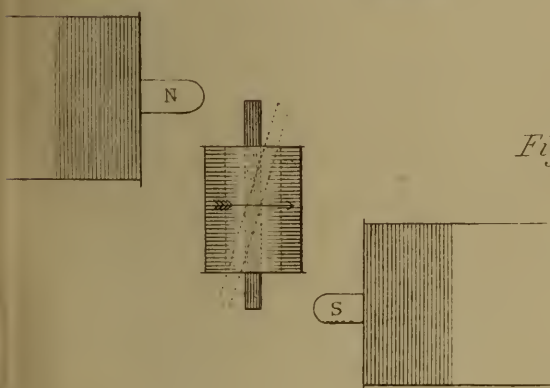


Fig. 41.



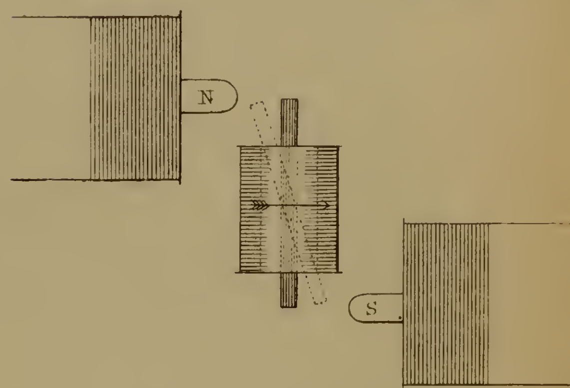
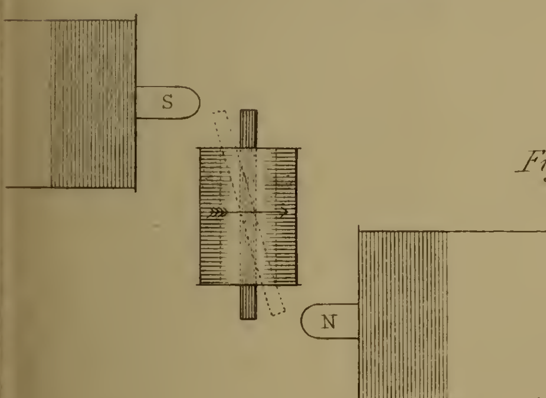


Fig. 42.



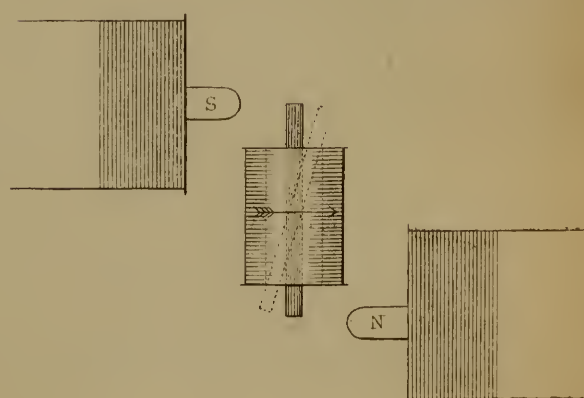
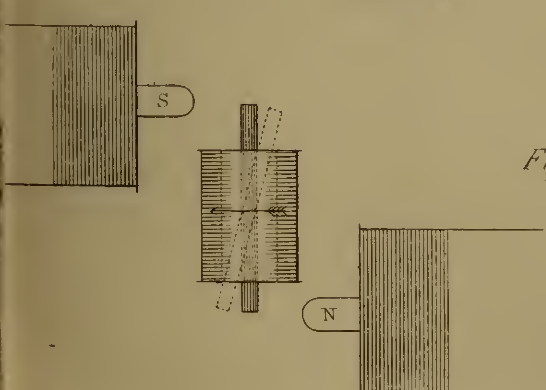
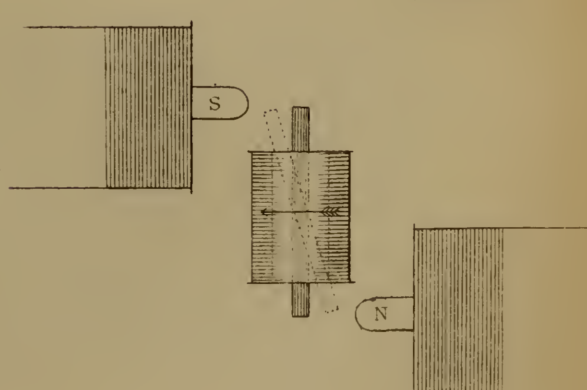


Fig. 43.





proceeded to the current reverser R′, and thence to a second battery of considerably less power than the former. By means of the reverser R′ the polarity of the cores could be changed; P′ could be converted from a south pole to a north pole, at the same time that P was converted from a north pole to a south pole. Lastly, by a change of the connexions between the two helices, the cores could be so excited as to make the poles of the same quality, both north or both south.

The diameter of the cylindrical space, within which the bismuth bar was suspended, was such as to permit of a free play of the ends of the bar through the space of an inch and a half. Having seen that the bar swung without impediment, and that its axis coincided as nearly as possible with the axis of the helix, a current from the battery was sent through the latter. The magnetism of the cores P and P′ was then excited, and the action upon the bismuth bar observed. M. v. Feilitzsch has attempted a similar experiment to that here described, but without success: when, however, sufficient power is combined with sufficient delicacy, the success is complete, and the most perfect mastery is obtained over the motions of the bar.

The helix above described surrounding the bismuth bar is the one which I have found most convenient for the experiments; various other helices, however, were tried, with a result equally certain, if less energetic. The one first made use of was 4 inches long, 3 inches exterior diameter, and three-quarters of an inch interior diameter, with wire one-fifteenth of an inch in thickness, the bar being suspended by a fibre which passed through a slit in the helix: sending through this helix a current from a battery of 10 cells, and exciting the cores by a current from 1 cell, the phenomena of repulsion and *attraction* were exhibited with all desirable precision.

I will now describe the results obtained by operating in the manner described. The bismuth bar being suitably suspended, a current was sent through the helix, so that the direction of the current *in the upper half* was that indicated by the arrow in fig. 40, Pl. II*a*. On exciting the magnet, so that the pole N was a north pole and the pole S a south pole, the ends of the bar of bismuth were *repelled*. The final position of the bar was against the side of the helix most remote from the magnets: it is shown by dots in the figure.

By means of the reverser R the current was now sent through the helix in the direction shown in fig. 41: the bar promptly left its position, crossed the space in which it could freely move, and came to rest as near the magnets as the side of the helix would permit it. *It was manifestly attracted by the magnets.*

Permitting the current in the helix to flow in the last direction, the polarity of the iron cores was reversed. We had then the state of things sketched in fig 42. The bismuth bar instantly loosed from the position it formerly occupied, receded from the magnet, and took up finally the position marked by the dots.

After this new position had been attained, the current through the helix was reversed: the bar promptly sailed across the field towards the magnets, and finally came to rest in the dotted position, fig. 43. In all these cases, when the bar was freely moving in any direction, under the operation of the forces acting upon it, the reversion either of the current in the helix or of the polarity of the cores arrested the motion; approach was converted into recession, and recession into approach.

The ends of the helix in these experiments were not far from the ends of the soft iron cores; and it might therefore be supposed that the action was due to some modification of the cores by the helix, or of the helix by the cores. It is manifest that the magnets can have no *permanent* effect upon the helix; the current through the latter, measured by a tangent galvanometer, is just as strong when the cores are excited as when they are unexcited. The helix may certainly have an effect upon the cores, and this effect is either to enfeeble the magnetism of the cores or to strengthen it; but if the former, and if the bar were the simple bismuth which it is when no current operates on it, the action, though weakened, *would still be repulsive*; and if the latter, the increase would simply augment the repulsion. The fact, however, of the ends of the bar being *attracted*, proves that the bar has been thrown into a peculiar condition by the current circulating in the surrounding coil. Changing the direction of the current in the coil, we find that the self-same magnetic forces which were formerly attractive are now repulsive; to produce this effect the condition of the bar must have changed with the change of the current; or, in other words, the bar is capable

of accepting *two different states* of excitement, which depend upon the direction of the current.

In order, however, to reduce as far as possible the action of the helix upon the cores, I repeated the experiments with the small helix referred to in fig. 6, page 117. It will be remembered that this helix is but an inch in length, and that the bismuth bar is $6\frac{1}{2}$ inches long. I removed the magnets further apart, so that the centres of the cores were half an inch beyond the ends of the bismuth bar, while the helix encircled only an inch of its central portion: in this position, when the helix was excited, there was no appreciable magnetism excited by it in the dormant cores; at least, if such were excited, it was unable to attract the smallest iron nail. Here then we had cores and helix sensibly independent of each other, but the phenomena appeared as before. The bar could be held by the cores against the side of the helix, with its ends only a quarter of an inch distant from the ends of the cores; on reversing either of the currents the ends instantly receded, but the recession could be stopped by again changing the direction of the current. With a tranquil atmosphere, and an arrangement for reversing the current without shock or motion, the bar obeyed in an admirable manner the will of the experimenter, and, under the operation of the forces indicated, exhibited all the deflections sketched in figs. 40, 41, 42, and 43.

The motion of the bar cannot be referred to the action of induced currents. The bar was brought into the centre of the hollow cylinder in which it swung, and held there; the forces were all in action, and therefore all phenomena of induction passed; the arrangement of the forces being that shown in fig. 40, on releasing the bar it was driven from the cores, whereas when the arrangement was that shown in fig. 41, it was drawn towards them.

But it does not sufficiently express the facts to say that the bar is capable of two different states of excitement; it must be added, that both states exist simultaneously in the excited bar. It has been already proved, that the state corresponding to the action of one pole is not that which enables an opposite pole to produce the same action; hence, when the two ends of the bar are attracted or repelled, at the same time, by two opposite poles, it is a proof that these two ends are in opposite states. But if

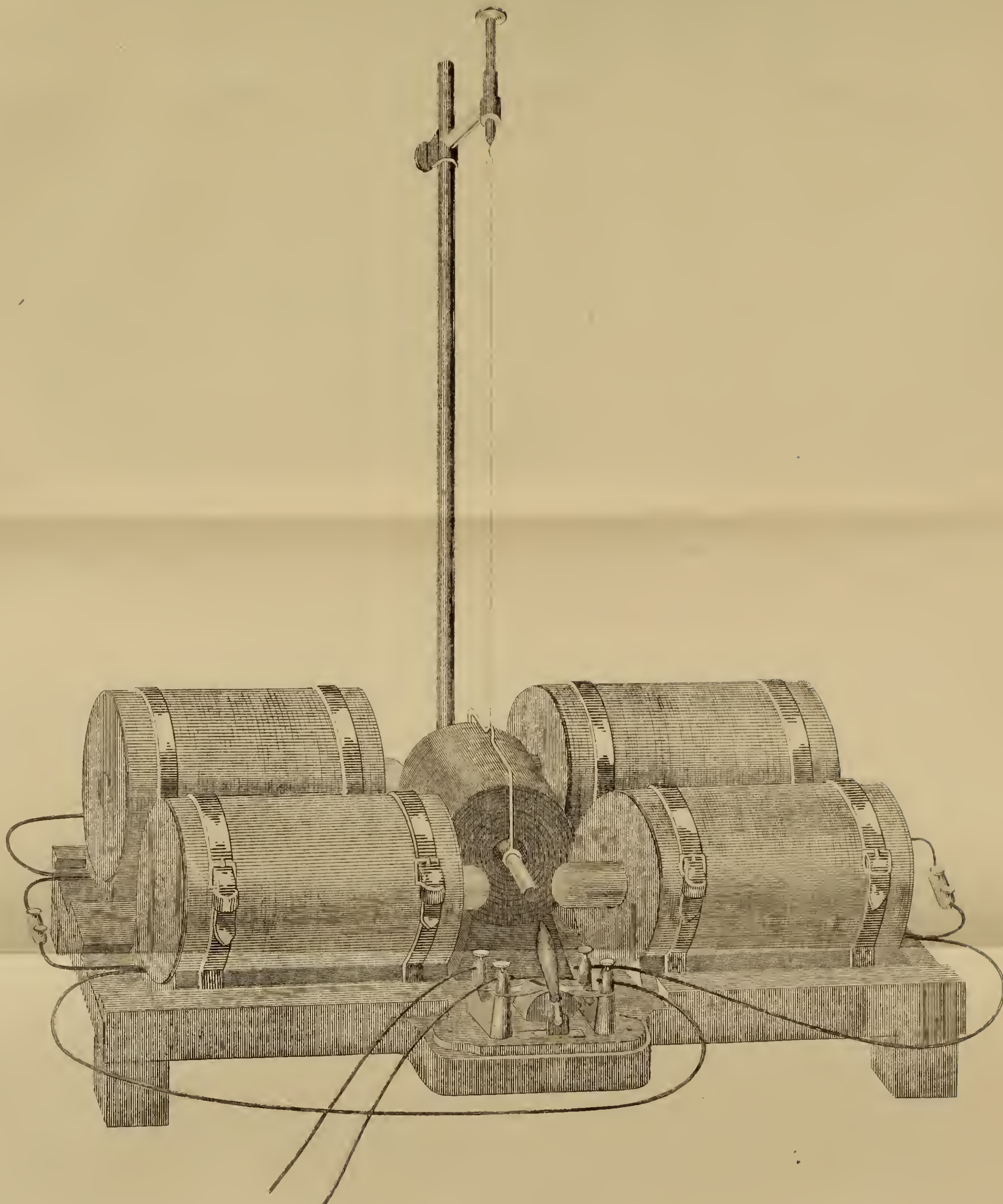
this be correct, we can test our conclusion by reversing one of the poles; the direction of its force being thereby changed, it ought to hold the other pole in check and prevent all motion in the bar. This is the case: if, in any one of the instances cited, the polarity of either of the cores be altered; if the south be converted into a north, or the north into a south pole, thus making both poles of the same quality, the repulsion of the one is so nearly balanced by the attraction of the other, that the bar remains without motion towards either of them.

To carry the argument a step further, let us fix our attention for an instant upon fig. 40. The end of the bar nearest to the reader is repelled by a south pole; the same end ought to be *attracted* by a north pole. In like manner, the end of the bar most distant from the reader is repelled by a north pole, and hence the state of that end ought to fit it for *attraction* by a south pole. If, therefore, our reasoning be correct, when we place a north pole opposite to the near end of the bar, and on the same side of it as the upper north pole, and a south pole opposite the further end of the bar and on the same side of it as the lower south pole, the simultaneous action of these four poles ought to be more prompt and energetic than when only two poles are used. This arrangement is shown in Plate III.: the two poles to the right of the bismuth bar must be of the same name, and the two to the left of the bar of the opposite quality. If those to the right be both north, those to the left must be both south, and *vice versâ*. The current reverser for the magnets appears in front, that for the helix is hidden by the figure. The above conclusion is perfectly verified by experiments with this apparatus, and the twofold deflection of the bismuth bar is exhibited with remarkable energy.*

The bar used in these cases is far heavier than those commonly made use of in experiments on diamagnetism, but the dimensions stated do not mark the practical limit of the size of the bar. A solid bismuth cylinder, 14 inches long and 1 inch in diameter, was suspended in a helix 5·7 inches long, 1·8 inch internal diameter, 4 inches external diameter, and composed of

* These experiments, and almost all the others mentioned in this memoir, may be exhibited in the lecture-room. By attaching indexes of wood to the bars of bismuth, and protecting the indexes from air currents by glass shades, the motions may be made visible to several hundreds at once. See a description of a Polymagnet, Phil. Mag. June 1855.—J. T.

Plate III



copper wire 0·1 of an inch in thickness. When a current of twenty cells was sent through the helix, and the magnets (only two of them were used) were excited by one cell, all the phenomena exhibited by figs. 40, 41, 42, and 43 were distinctly exhibited.

A considerable difference is always necessary between the strength of the current which excites the bismuth and that which excites the cores, so as to prevent the induction of the cores, which of itself would be followed by repulsion, from neutralising, or perhaps inverting, the induction of the helix. When *two* magnets were used and the bismuth excited by ten cells, I found the magnetic excitement by one or two cells to be most advantageous. When the cores were excited by ten, or even five cells, the action was always repulsive.* When four magnets were applied and the bismuth was excited by a battery of ten or fifteen cells, a power of five cells for the magnets was found efficient.

The deportment of paramagnetic bodies is so well known, that it might be left to the reader to discern that in all the cases described it is perfectly antithetical to that of the diamagnetic body. I have nevertheless thought it worth while to make the corresponding experiments with an iron bar; and to facilitate comparison, the results are placed in Plate IV. side by side with those obtained with the bar of bismuth. It must be left to the reader to decide whether throughout this inquiry the path of strict inductive reasoning has not been adhered to: if this be the case, then the inference appears unavoidable,

That the diamagnetic force is a polar force, the polarity of diamagnetic bodies being opposed to that of paramagnetic ones under the same conditions of excitement.†

* The perfect similarity of this deportment to that of soft iron under the same circumstances is evident.

† I would gladly refer to M. Plücker's results in connexion with this subject had I been successful in obtaining them; I will here, however, introduce the description of his most decisive experiment in his own words. (See Scien. Mem. New Ser. p. 336.)

'From considerations of which we shall speak afterwards, it appeared to me probable that bismuth not only assumes polarity in the vicinity of a magnetic pole, but that it also retains the polarity for some time after the excitation has taken place; or, in other words, that bismuth retains a portion of its magnetism permanently, as steel, unlike soft iron, retains a portion of the magnetism excited in it by induction. My conjecture has been corroborated by experiment.

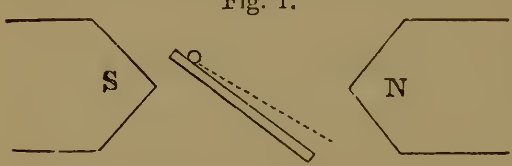
'I hung a bar of bismuth, 15 millims. long and 5 millims. thick, between the pointed poles of the large electro-magnet; it was suspended horizontally from a

VI. On M. Weber's Theory of Diamagnetic Polarity,* and on Ampère's Theory of Molecular Currents.

If we reflect upon the experiments recorded in the foregoing pages from first to last; on the inversion of magne-crystallic phenomena by the substitution of a magnetic constituent for a diamagnetic; on the analogy of the effects produced in magnetic and diamagnetic bodies by compression; on the antithesis of the rotating actions described near the commencement; on the indubitable fact that diamagnetic bodies, like magnetic ones, owe their phenomena to an induced condition into which they are thrown by the influencing magnet, and the intensity of which is a function of the magnetic strength; on the circumstance that

double cocoon-thread, fig. 1. The distance between the points was diminished until the bar could barely swing freely between them. A little rod of glass was brought near to one of the points, so that the bismuth bar, before the magnetism was excited, and in consequence of the torsion, leaned against the glass rod. On exciting the magnet by current of three of Grove's elements, the bismuth, prevented from assuming the equatorial position, pressed more forcibly against the glass rod; when the current was interrupted, the bar remained still in contact with the rod, while its free end vibrated round its position of equilibrium. The current was closed anew and then reversed by a gyrotrope. In consequence of this reversion, the bar of bismuth, loosening from the glass rod, moved towards the axial position, but soon turned and pressed against the glass as before, or in some cases having passed quite through the axial position was driven round with the reversed ends into the equatorial. . . . . This experiment, which was made with some care, proves that the bismuth requires time to reverse its polarity.'

Fig. 1.



I have repeated this experiment with great care, and have obtained in part the effect described: it is perfectly easy to produce the rotation of the bar. The cause of this rotation, however, was in my case as follows:—When the magnet was unexcited, the position of equilibrium of the axis of the bar acted upon by the torsion of the fibre was that shown by the dotted line in the figure; when the magnetism was developed, the repulsive force acting on the free end of the bar necessarily pushed it beyond the dotted line—an action which was perfectly evident when the attention was directed towards it. On reversing the current, a little time was required to change the polarity of the iron masses; during this time the free end of the bismuth *fell* towards its former position, and the velocity required was sufficient to carry it quite beyond the pole points. The only difference between M. Plücker and myself is, that I obtained the same result by simply *intercepting* the current as by reversing it. I may remark that I have submitted ordinary bismuth to the most powerful and delicate tests, but as yet I have never been able to detect in it a trace of that retentive power ascribed to it by M. Plücker.

* Poggendorff's *Annalen*, vol. lxxxvii. p. 145, and Taylor's Scientific Memoirs, New Ser. p. 163.

this excitation, like that of soft iron, is of a dual character; on the numerous additional experiments which have been recorded, all tending to show the perfect antithesis between the two classes of bodies; we can hardly fail to be convinced that Mr. Faraday's first hypothesis of diamagnetic action is the true one—that diamagnetic bodies operated on by magnetic forces possess a polarity 'the same in kind as, but the reverse in direction of that acquired by magnetic bodies.' But if this be the case, how are we to conceive of the *physical mechanism* of this polarity? According to Coulomb's and Poisson's theory, the act of magnetisation consists in the decomposition of a neutral magnetic fluid; the north pole of a magnet, for example, possesses an attraction for the south fluid of a soft piece of iron submitted to its influence, draws the said fluid towards it, and with it the material particles with which the fluid is associated. To account for diamagnetic phenomena this theory seems to fail altogether; according to it, indeed, the oft-used phrase, 'a north pole exciting a north pole, and a south pole a south pole,' involves a contradiction. For if the north fluid be supposed to be *attracted* towards the influencing north pole, it is absurd to suppose that its presence there could produce *repulsion*. The theory of Ampère is equally at a loss to explain diamagnetic action; for if we suppose the particles of bismuth surrounded by molecular currents, then according to all that is known of electro-dynamic laws, these currents would set themselves parallel to, and in the same direction as those of the magnet, and hence attraction, and not repulsion, would be the result. The fact, however, of this not being the case proves that these molecular currents are not the mechanism by which diamagnetic induction is effected. The consciousness of this, I doubt not, drove M. Weber to the assumption that the phenomena of diamagnetism are produced by molecular currents, not *directed*, but actually *excited* in the bismuth by the magnet. Such induced currents would, according to known laws, have a direction *opposed* to those of the inducing magnet, and hence would produce the phenomena of repulsion. To carry out the assumption here made, M. Weber is obliged to suppose that the molecules of diamagnetic bodies are surrounded by channels, in which the induced molecular currents, once excited, continue to flow without resistance.

This theory, notwithstanding its great beauty, is so extremely

artificial, that I imagine the general conviction of its truth cannot be very strong; but there is one conclusion flowing from it which appears to me to be in direct opposition to experimental facts. The conclusion is '*that the magnetism of two iron particles in the line of magnetisation is increased by their reciprocal action; but that, on the contrary, the diamagnetism of two bismuth particles lying in this direction is diminished by their reciprocal action.*' The reciprocal action of the particles varies inversely as the cube of the distance between them; at a distance expressed by the number 1, for example, the enfeeblement is eight times what it would be at the distance 2.

The conclusion, as regards the iron, is undoubtedly correct; but I believe experiment proves that the mutual action of diamagnetic molecules, when caused to approach each other, *increases* their repulsive action. I have had massive iron moulds* made and coated with copper electrolytically; into these fine bismuth powder has been introduced and submitted to powerful hydraulic pressure. No sensible fact can, I think, be more certain, than that the particles of this dust are brought into closer proximity along the line in which the pressure is exerted, and this is the line of *strongest diamagnetisation.* If a portion of the compressed mass be placed upon the end of a torsion beam and the amount of repulsion measured, it will be found that the repulsion is a maximum when the line of magnetisation coincides with the line of compression; or, in other words, with that line in which the particles are packed most closely together; if the bismuth were fixed, and the magnet movable, the former would repel the latter with a maximum force when the line of compression is parallel to the direction of magnetisation. It is a stronger *diamagnet* in this direction than in any other. Cubes of bismuth which, in virtue of their crystallisation, possessed a line of minimum magnetisation, have been placed in those moulds and pressed closely together in the direction of the said line: the approximation of the particles thus affected has converted the direction spoken of from one of minimum into one of maximum magnetisation. It would be difficult for me to say how many diamagnetic bodies I have submitted to compression, some massive, some in a state of powder, but in no single instance

* For drawings of these moulds see page 262.

have I discovered an exception to the law that the line of compression of purely diamagnetic bodies is the line of strongest diamagnetisation. The approximation of diamagnetic particles is therefore accompanied by an augmentation of their power, instead of a diminution of it, as supposed by the theory of M. Weber.

It is scarcely possible to reflect upon the discovery of Faraday in all its bearings, without being deeply impressed with the feeling that we know absolutely nothing of the physical causes of magnetic action. We find the magnetic force producing, by processes which are evidently similar, two great classes of effects. We have a certain number of bodies which are attracted by the magnet, and a far greater number which are repelled by the same agent. Supposing these facts to have been known to Ampère, would he have satisfied his profound mind by founding a theory which accounts for only the smaller portion of them? This theory is admirable as far as it goes, but the generalisation is yet to come which shall show the true relationship of phenomena, towards whose connection the theory of Ampère furnishes at present no apparent clue.

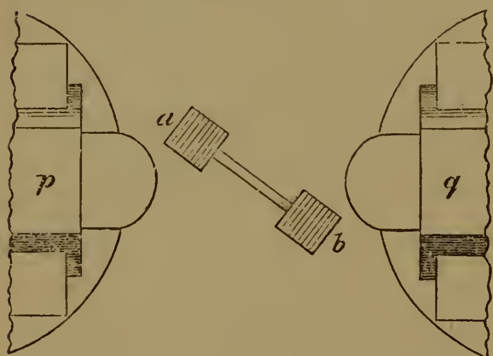
On M. Matteucci's Objections.

The foregoing memoir was on the point of leaving my hands for the Royal Society, when accident, backed by the kindness of Mr. Faraday, placed the *Cours spécial* of M. Matteucci, recently published in Paris, in my hands. An evening's perusal of this valuable work induces me to append the following remarks to the present paper.

M. Matteucci honours the researches which bear my name, and those which I published in connection with M. Knoblauch, with a considerable share of his attention. He corroborates all the experimental facts, but at the conclusion states three objections to the manner in which these facts have been explained. 'La faveur,' writes the learned Italian, 'avec laquelle les idées de MM. Tyndall et Knoblauch ont été accueillies m'imposent le devoir de ne pas vous laisser ignorer les objections qui s'élèvent contre elles. La première consiste dans la différence très-grande et constante dans la force qui fait osciller entre les pôles une aiguille de bismuth cristallisé, suivant que ses clivages parallèles à sa longueur sont suspendus verticalement ou dans un plan

horizontal: cette différence me paraît inconciliable avec le résultat déjà rapporté de l'expérience de M. Tyndall, sur lequel se fonde l'explication des phénomènes magnéto-cristallisés. Mais une objection encore plus grave est celle du mouvement d'*attraction** vers les pôles qui se manifeste dans les prismes de bismuth cristallisé dont les clivages sont perpendiculaires à leur longueur. Pour rendre la conséquence de cette dernière expérience encore plus évidente, j'ai fixé deux cubes de bismuth, qui ont deux faces opposées naturelles et parallèles aux plans de clivage, aux extrémités d'un petit levier de verre, ou de sulphate de chaux, suspendu par un fil de cocon au milieu du champ magnétique entre les extrémités polaires d'un électro-aimant (fig. 27*a*); lorsque les deux cubes ont les clivages verticaux et perpendiculaires à la longueur de l'aiguille, au moment où le circuit est fermé, l'aiguille est attirée, *quelle que soit la position* qu'elle occupe dans le champ magnétique, et se fixe en équilibre dans la ligne polaire. . . . . Il me semble impossible d'expliquer ces mouvements du bismuth cristallisé, comme on a essayé de le faire, par la force répulsive de l'aimant, qui, suivant l'expérience de M. Tyndall,† s'exerce avec plus d'intensité parallèlement aux clivages que dans la direction perpendiculaire à ces plans.

Fig. 27*a*.



'Remarquons encore qu'on ne trouve pas constamment l'accord qui devrait exister, selon les idées de MM. Tyndall et Knoblauch, entre les phénomènes magnéto-cristallisés et les effets produits par la compression dans le bismuth, si l'on considère ces plans de clivage et la ligne suivant laquelle la compression a eu lieu comme jouissant des mêmes propriétés.'‡

With regard to the first objection I may say that it is extremely difficult to meet one so put; it is simply an opinion,

* This is in reality not a 'movement of *attraction*.'—See Appendix to the present paper.—J. T., May 1855.

† This was first proved by Mr. Faraday.—J. T.

‡ *Cours spécial sur l'Induction, etc.*, p. 255.

and I can scarcely say more than that mine does not coincide with it. I would gladly enter upon the subject and endeavour to give the objection a scientific form were the necessary time at my disposal, but this, I regret to say, is not the case at present. I shall moreover be better pleased to deal with the objection after it has assumed a more definite form in the hands of its proposer, for I entertain no doubt that it is capable of a sufficient answer. The second objection M. Matteucci considers to be a more grave one. The facts are as follows:—the repulsion of a mass of crystallised bismuth depends upon the direction in which the mass is magnetised. When the magnetising force acts in a certain direction, the intensity of magnetisation, and the consequent repulsion of the mass, is a maximum. This is proved by placing the mass upon the end of a torsion beam and bringing its several directions successively into the line of the magnetic force. Poisson would have called such a direction through the mass a principal axis of magnetic induction, and it has been elsewhere called a line of elective polarity. When a sphere or cube of bismuth is freely suspended in the magnetic field, with the direction referred to horizontal, in all positions except two the forces acting on the mass tend to turn it; those positions are, when the line of maximum magnetisation is axial and when it is equatorial, the former being a position of unstable, and the latter a position of stable equilibrium. When the above line is oblique to the direction of magnetisation, the sphere or cube will turn round its axis of suspension until the direction referred to has set itself at right angles to the line joining the poles. Now if the direction of maximum magnetisation be transverse to an elongated mass of bismuth, such a mass must, when the said direction recedes to the equator, set its length from pole to pole. The facts observed by M. Matteucci seem to me to be a simple corroboration of this deduction.

The third objection is directed against an imaginary case, 'si l'on considère les plans de clivage et la ligne de compression comme jouissant des mêmes propriétés.' It must be evident that a crystal like bismuth, possessing a number of cleavages of unequal values, cannot be compared in all respects with a body which has suffered pressure *in one direction* only. I have no doubt whatever, that, by a proper application of

force in different directions, a compressed mass might be caused to imitate to perfection every one of the actions exhibited by crystallised bismuth. Indeed, I would go further, and say, that I shall be happy to undertake to reproduce, with bismuth powder, the deportment of any diamagnetic crystal whatever that M. Matteucci may think proper to name.

In looking further over M. Matteucci's instructive book, I find another point alluded to in a manner which tempts me to make a few remarks in anticipation of a fuller examination of the subject. The point refers to the reciprocal action of the particles of magnetic and diamagnetic bodies. It is easy to see, that if the attraction of a bar of iron varies simply as the number of the particles attracted, then, inasmuch as the weight of the body varies in the same ratio, and the moment of inertia as the weight, the times of oscillation of two masses of the same length, but possessing different numbers of attracting particles, must be the same. Coulomb indeed mixed iron filings with wax, so as to remove the particles out of the sphere of their mutual inductive action, and proved that when needles of equal lengths, but of different diameters, were formed from the same mixture, the duration of an oscillation was the same for all. From this he inferred that the attractive force is simply proportional to the number of ferruginous particles; but this could not be the case if these particles exerted any sensible reciprocal action, either tending to augment or diminish the induction due to the direct action of the magnet. On account of such a mutual action, two bars of solid iron, of the same length, and of different diameters, have not the same time of oscillation.

In examining the question whether the particles of diamagnetic bodies exert a similar reciprocal action, M. Matteucci fills quills of the same length, and of different diameters, with powdered bismuth, and finds that there is no difference between the duration of an oscillation of the thick ones and the slender ones; from this he infers that there can be no reciprocal action among the particles of the bismuth.

Now it is not to be imagined that even in Coulomb's experiments with the iron filings the molecular induction was absolutely nothing, but simply that it was so enfeebled by the separation of the particles that it was insensible in the ex-

periments. This remark applies with still greater force to M. Matteucci's experiments with the bismuth powder; for the enfeeblement of a force already so weak, by the division of the diamagnetic mass into powder, must of course practically extinguish all reciprocal action of the particles, even supposing a weak action of the kind to exist when the mass is compact.

I will not here refer to my own experiments on compressed bismuth, but will take a result arrived at by M. Matteucci himself while repeating and corroborating these experiments. 'I made,' says M. Matteucci, 'two cylinders of bismuth precisely of the same dimensions, the one compressed, the other in its natural state, and found that the compressed mass had a diamagnetic power *distinctly superior* to that of natural bismuth.'* Now M. Matteucci, in his *Cours spécial*, has made his own choice of a test of reciprocal molecular action; he assumes that if cylinders of the same length, but of different masses, have equal times of oscillation, it is a conclusive proof that there is no action of the kind referred to. This necessarily implies the assumption, that were the times of oscillation *different*, a reciprocal action would be demonstrated. According to his experiments described in the Association Report, the times of oscillation *are* different; the diamagnetism of the compressed cylinder is '*distinctly superior*' to that of the uncompressed one: *the diamagnetic effect increases in a greater proportion than the quantity of matter*; and hence, on M. Matteucci's own principles, the result negatived by his experiments on powdered bismuth is fairly established by those which he has made with the compressed substance.

FURTHER REFLECTIONS.†

Reflecting further on the subject of diamagnetic polarity, an experiment occurred to me which constitutes a crucial test to which the conclusions arrived at in the foregoing memoir may be submitted.

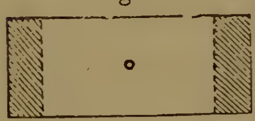
Two square prisms of bismuth, 0·43 of an inch long and 0·2 of an inch wide, were laid across the ends of a thin plate of cedar wood, and fastened there by white wax. Another similar

* Report of British Association for 1852, Transactions of Sections, p. 7.

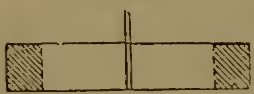
† Received December 21, 1854.

plate of wood was laid over the prisms, and also attached to them by wax; a kind of rectangular box was thus formed, 1 inch long and of the same width as the length of the prisms, the ends of the box being formed by the prisms, while its sides were open. Both plates of wood were pierced through at the centre, and in the aperture thus formed a wooden pin was fixed, which could readily be attached to a suspending fibre. Fig. 1 represents the arrangement both in plan and section.

Fig. 1.



Section.



The prisms first chosen were produced by the compression of fine bismuth powder, without the admixture of gum or any other foreign ingredient, the compressed mass being perfectly compact and presenting a surface of metallic brilliancy. Placed on the end of a torsion balance, with a magnetic pole brought to bear upon it, the repulsion of such a mass is a maximum when the direction in which the mass has been squeezed is in the continuation of the axis of the magnet. A comparative view of the repulsion in this direction, and in another perpendicular to it, is given in the following Table:—

Compressed Bismuth powder.

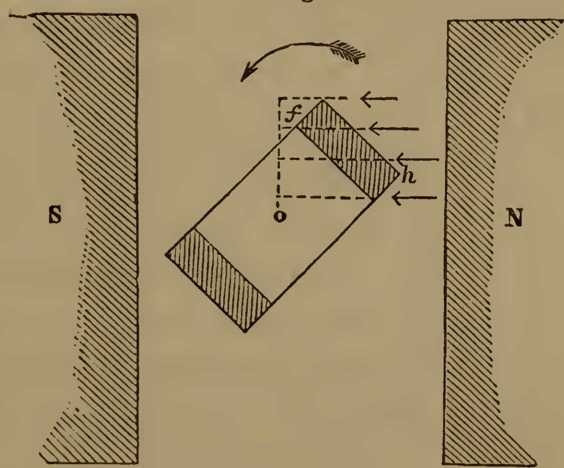
Strength of magnet.	Repulsion. Line of pressure axial.	Repulsion. Line of pressure equatorial.
5·8	22	13
8·4	46	31
10·0	67	46
11·9	98	67

We see here that the repulsion, when the line of pressure is axial, exceeds what occurs when the same line is equatorial by fully one-half the amount of the latter. Now this can only be due to the more intense magnetisation, or rather diamagnetisation, of the bismuth along the line of pressure; and in the experiment now to be described, I availed myself of this fact to render the effect more decided.

The prisms of bismuth were so constructed that the line of pressure was parallel to the *length* of each. The rectangular box above referred to was suspended from its centre of gravity in the magnetic field, so that the two prisms were in the same horizontal plane. Let the position of the box thus suspended be that shown in fig. 2. For the sake of

simplicity, we will confine our attention to the action of one of the poles N, which may be either flat or rounded, upon the prism *hf* adjacent to it, as indeed all the phenomena to be described can be produced before a single pole. The direction of the force emanating from N is represented by the arrows; and if this force be *purely repulsive,* the action upon every single particle of the diamagnetic mass furnishes a ‘moment’ which, in the position here assumed, tends to turn the rectangular box in the direction marked by the arrow. *It is perfectly impossible that such a system of forces could cause the box to turn in a direction opposed to the arrow ; yet this is the direction in which the box turns when the magnetic force is developed.*

Fig. 2.

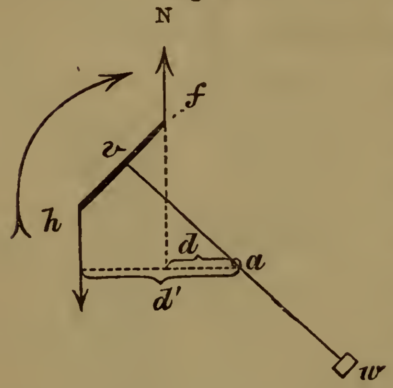


Here, then, we have a mechanical effect which is absolutely inexplicable on the supposition that the diamagnetic force is purely repulsive. But if the conclusions arrived at in the foregoing memoir be correct, if the diamagnetic force be a polar force, then we must assume that attraction and repulsion are developed simultaneously, as in the case of ordinary magnetic phenomena. Let us examine how this assumption will affect the analysis of the experiment before us.

The marked end of a magnetic needle is pulled towards the north magnetic pole of the earth; and yet, if the needle be caused to float upon a liquid, there is no motion of its mass towards the terrestrial pole referred to. The reason of this is known to be, that the south end of the needle is repelled by a force equal to that by which the north, or marked end, is attracted. These two equal and opposite forces destroy each other as regards *a motion of translation,* but they are effective in producing *a motion of rotation.* The magnetic needle, indeed, when in a position oblique to the plane of the magnetic meridian, is solicited towards that plane by a mechanical couple, and if free to move, will turn and find its position of equilibrium there.

Let such a needle, fh, be attached, as in fig. 3, to the end of a light wooden beam, vw; let the beam and needle be suspended horizontally from the point a, round which the whole system is free to turn, the weight of the needle being balanced by a suitable counterpoise, w; let the north pole of the earth be towards N. Supposing the beam to occupy a position oblique to the magnet meridian, as in the figure, the end f, or the marked end, of the needle is solicited towards N by a force ϕ, and the tendency of this force to produce rotation in the direction of the arrow is expressed by the product of ϕ into the perpendicular drawn from the centre of suspension a to the line of direction of the force. Setting this distance $= d$, we have the moment of ϕ in the direction stated,

Fig. 3.



$$=\phi d.$$

The end h of the needle is repelled by the magnetic pole N with a force ϕ': calling the distance of the direction of this latter force from the axis of rotation, d', we have the moment of ϕ' in a direction opposed to the arrow,

$$=\phi' d'.$$

Now as the length of the needle may be considered a vanishing quantity as compared with its distance from the terrestrial pole, we have practically

$$\phi = \phi',$$

and consequently, as d is less than d',

$$\phi d < \phi' d'.$$

The tendency to turn the lever in a direction opposed to the arrow is therefore predominant; the lever will obey this tendency, and move until the needle finds itself in the magnetic meridian; when this position is attained, the predominance spoken of evidently ceases, and the system will be in equilibrium. Experiment perfectly corroborates this theoretic deduction.

In this case, the centre of gravity of the needle recedes from the north magnetic pole as if it were repelled by the latter; but it is evident that the recession is not due either to the attraction

or repulsion of the needle considered as a whole, but simply to the mechanical advantage possessed by the force ϕ', on account of its greater distance from the axis of rotation. If the force acting upon every particle of the needle were purely *attractive*, it is evident that no such recession could take place. Supposing, then, that we were simply acquainted with the fact, that the end f of the needle is attracted by the terrestrial pole, and that we were wholly ignorant of the action of the said pole upon the end h, the experiment here described *would lead us infallibly to the conclusion that the end h must be repelled.* For if it were attracted, or even if it were neither attracted nor repelled, the motion of the bar must be *towards* the pole N instead of in the opposite direction.

Let us apply this reasoning to the experiment with the bismuth prisms already described. The motion of the magnetic needle in the case referred to is not more inexplicable, on the assumption of a purely attractive force, than is the motion of our rectangular box on the assumption of a purely repulsive one; and if the above experiment would lead to the conclusion that the end h of the magnetic needle is repelled, the experiment with the bismuth leads equally to the conclusion that the end f of the prism hf, fig. 2, must be *attracted* by the pole N. *The assumption of such an attraction, or in other words, of diamagnetic polarity, is alone capable of explaining the effect, and the explanation which it offers is perfect.*

On the hypothesis of diamagnetic polarity, the prism hf turns a hostile end h to the magnetic pole N, and a friendly pole f away from it. Let the repulsive force acting upon the former be ϕ, and the attractive force acting upon the latter ϕ'. It is manifest that if ϕ were equal to ϕ', as in the case of the earth's action, or in other words, if the field of force were perfectly uniform, then, owing to the greater distance of ϕ' from the axis of rotation, from the moment at which the rectangular box quits the equatorial position, which is one of unstable equilibrium, to the moment when its position is axial, the box would be incessantly drawn towards the position last referred to.

But it will be retorted that the field of force is not uniform, and that the end h, on account of its greater proximity to the magnet, is more forcibly repelled than the end f is attracted: to this I would reply, that it is only in 'fields' which are approxi-

mately uniform that the effects can be produced; but to produce motion towards the pole, it is not necessary that the field should be perfectly uniform: setting, as before, the distance of the direction of the force ϕ from the axis of rotation $= d$, and that of the force $\phi' = d'$, a motion towards the pole N will always occur whenever

$$\frac{d'}{d} > \frac{\phi}{\phi'}.$$

To ascertain the diminution of the force on receding from a polar surface such as that here used, I suspended a prism of bismuth, similar to those contained in the rectangular box, at a distance of 0·9 of an inch from the surface of the pole. Here, under the action of the magnet excited by a current of ten cells, the number of oscillations accomplished in a second was 17; at 0·7 of an inch distant the number was 18; at 0·5 of an inch distant the number was 19; at 0·3 distant the number was 19·5; and at 0·2 distant the number was 20. The forces at these respective distances being so very little different from each other, it follows that a very slight deviation of the box from the equatorial position is sufficient to give the moment of ϕ' a preponderance over that of ϕ, and consequently to produce the exact effect observed in the experiment.

The consistency of this reasoning is still further shown when we operate in a field of force which diminishes speedily in intensity as we recede from the magnet. Such a field is the space immediately in front of pointed poles. Suspending our rectangular box between the points, and causing the latter to approach until the box has barely room to swing between them, it is impossible to produce the phenomena which we have just described. The intensity with which the nearest points of the bismuth bar are repelled so much exceeds the attraction of the more distant end, that the moment of attraction is not able to cope successfully with the moment of repulsion; the bars are consequently repelled *en masse*, and the length of the box takes up a position at right angles to the line which unites the poles.

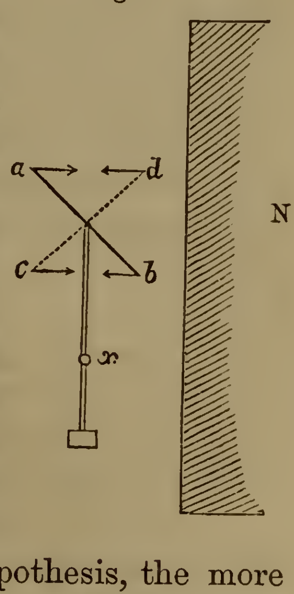
It is manifest, however, that by increasing the distance between the bismuth bar and the points acting upon it, we diminish the difference of action upon the two ends of the bar. When the distance is sufficient, we can produce, with the pointed poles, all the phenomena exhibited between flat or rounded ones.

All the effects which have been described are produced with great distinctness when, instead of compressed bismuth, two similar bars of the crystallised substance are used, in which the planes of principal cleavage are parallel to the length. Such bars are not difficult to procure, and they ought to hang in the magnetic field with the planes of cleavage vertical. It is unnecessary to describe the experiments made with such bars; they exhibit with promptness and decision all the effects observed with the compressed bismuth.

We have hitherto operated upon elongated masses of bismuth; but with the compressed substance, or with the substance crystallised uniformly in planes, as in the case last referred to, an elongation of the mass is not necessary to the production of the effects described. Previous, however, to the demonstration of this proposition, I shall introduce a kind of lemma, which will prepare the way for the complete proof.

Diamagnetic bodies, like paramagnetic ones, vary considerably in the intensity of their forces. Bismuth or antimony, for example, exhibits the diamagnetic force with greater energy than gold or silver, just as iron or nickel exhibits the magnetic force with greater energy than platinum or chromium. Let two thin bars, *ab*, *cd*, fig. 4, of two bodies of different diamagnetic powers be placed at right angles to each other, so as to form a cross; let the cross be attached to the end of a lever and suspended horizontally from the point *x*, before the flat or rounded pole N of a magnet. Let the continuous line *ab* represent the needle of the powerful diamagnetic body, and the broken line *cd* that of the feeble one. On the former a mechanical couple acts in the directions denoted by the arrows at its ends; and on the latter a couple operates in the directions of the arrows at *its* ends. These two couples are evidently opposed to each other; but the former being, by hypothesis, the more powerful of the two, it will overcome the latter. The mechanical advantage possessed by the *attracted* end *a* of the more powerful bar, on account of its greater distance from the axis

Fig. 4.



of suspension x, will, in an approximately uniform field of force which we here assume, cause the centre of gravity of the cross to move towards the pole N.

In the formation of such a cross, however, it is not necessary to resort to two different substances in order to find two needles of different diamagnetic powers; for in crystallised bodies, or in bodies subjected to mechanical pressure, the diamagnetic force acts with very different energies in different directions. Let a mass of a diamagnetic body which has been forcibly compressed in one direction be imagined; let two needles be taken from such a mass, the one with its length parallel, and the other with its length perpendicular to the line of pressure. Two such needles, though composed of the same chemical substance, will behave exactly as the two bars of the cross in the experiment last described: that needle whose length coincides with the line of pressure will bear the same relation to the other that the needle of the powerfully diamagnetic substance bears to that of the feeble one. An inspection of the table at page 144 will show that this must be the case.

It is also shown in the following table, that in masses of crystallised bismuth the diamagnetic repulsion acts with very different energies in different directions. From a bismuth crystal cubes were taken with the planes of principal cleavage parallel throughout to two opposite faces of each cube. The cubes were placed upon the ends of a torsion balance, and the diamagnetic repulsion was accurately measured when the force acted parallel to the planes of cleavage. The cubes were then turned 90° round, and the repulsion was measured when the force acted perpendicular to the planes referred to.

Cubes of crystallised Bismuth.

	Repulsion when the force was directed	
Strength of magnet.	along the cleavage.	across the cleavage.
3·6	11·7	8
5·7	34·8	23
8·4	78	53
10·0	111	76·5
11·9	153	110

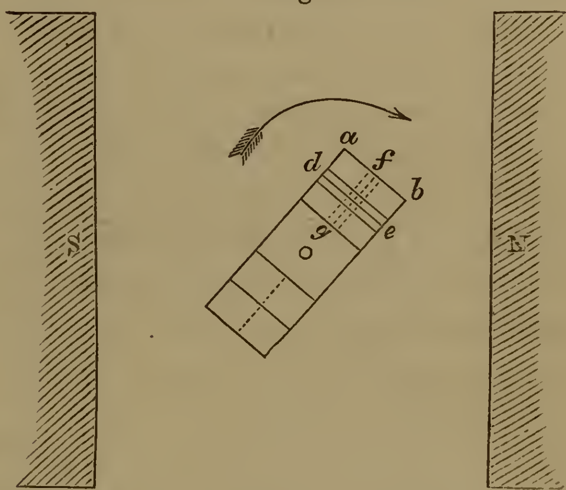
It is manifest from this table that bismuth behaves as a body of considerably superior diamagnetic power when the force acts *along* the planes of cleavage.

Let two indefinitely thin needles be taken from such a mass,

the one with its length parallel, and the other with its length perpendicular to the planes of cleavage; it is evident that if two such needles be formed into a cross and subjected to experiment in the manner above described, the former will act the part of the more powerfully diamagnetic needle, and produce similar effects in the magnetic field.

We now pass on to the demonstration of the proposition, that it is not necessary that the crystallised masses should be elongated to produce the effects exhibited by the prisms in the experiments already recorded. Let us suppose the ends of our rectangular box to be composed of cubes, instead of elongated masses, of crystallised bismuth, and let the planes of principal cleavage be supposed to be parallel to the face *ab*, fig. 5. Let the continuous line *de* represent an indefinitely thin slice of the cube passing through its centre, and the dotted line *gf* a similar slice in a perpendicular direction. These two slices manifestly represent the case of the cross in fig. 4; and were they alone active, the rectangular box, in a uniform field of magnetic force, must turn in the direction of the arrow. Comparing similar slices, *in pairs*, on each side of those two central slices, it is manifest that every pair parallel to the line *de* represents a stronger mechanical couple than every corresponding pair parallel to *fg*. The consequence is, that a cube of crystallised bismuth suspended in the manner described, in a sufficiently uniform field of magnetic force, will move in the same direction as the cross in fig. 4: its centre of gravity will therefore *approach* the pole N, which was to be demonstrated.

Fig. 5.



This deduction is perfectly illustrated by experiment. It is manifest that the effect of the pole S upon the cube adjacent to it is to increase the moment of rotation of the rectangular box: the same reasoning applies to it as to the pole N.

Referring to fig. 27*a*, page 140, it will be seen that we have here dealt with the second and gravest objection of M. Matteucci, and converted the facts upon which the objection is based into a proof of diamagnetic polarity, so cogent that it alone would seem to be sufficient to decide this important question.

Holding the opinion entertained by M. Matteucci regarding the nature of diamagnetic force,* his objection must have appeared to him to be absolutely unanswerable: I should be glad to believe that the remarks contained in this Appendix furnish, in the estimation of this distinguished philosopher, a satisfactory explanation of the difficulty which he has disclosed.

Let me, in conclusion, briefly direct the reader's attention to the body of evidence laid before him in the foregoing pages. It has been proved that matter is repelled by the pole of a magnet in virtue of an induced condition into which the matter is thrown by such a pole. It is shown that the condition evoked by one pole is not that which is evoked by a pole of an opposite quality—that each pole excites a condition peculiar to itself. A perfect antithesis has been shown to exist between the deportment of paramagnetic and diamagnetic bodies when acted on by a magnet alone, by an electric current alone, or by a magnet and an electric current combined. The perplexing phenomena resulting from molecular structure have been laid open, and the antithesis between paramagnetic and diamagnetic action traced throughout. It is further shown, that whatever title to polarity the deportment of a bar of soft iron, surrounded by an electric current, and acted on by other magnets, gives to this substance, a bar of bismuth possesses precisely the same title: the disposition of forces, which in the former case produces attraction, produces in the latter case repulsion, while the repulsion of the iron finds its exact complement in the attraction of the bismuth. Finally, we have a case adduced by M. Matteucci which suggests a crucial experiment to which all our previous reasoning has been submitted, by which its accuracy has been proved, and the insufficiency of the assumption, that the diamagnetic force is not polar, is reduced to demonstration. When we remember that against all this no single experimental fact† or theoretic

* "Il ne peut exister dans les corps diamagnétiques une polarité telle qu'on la conçoit dans le fer doux."—*Cours spécial*, p. 201.

† I refrain from alluding to the negative results obtained by Mr. Faraday in repeating

argument which can in any degree be considered as conclusive, has ever been brought forward, nor do I believe can be brought forward, the conclusion seems irresistible, that we have in the agency by which bodies are repelled from the poles of a magnet, a force of the same dual character as that by which bodies are attracted; that, in short, 'diamagnetic bodies possess a polarity the same in kind but the opposite in direction to that possessed by magnetic ones."

[The experiments and reasonings recorded in the foregoing memoir left no shadow of doubt upon my mind as to the polar character of the diamagnetic force. Throughout the most complex series of actions, the doubleness of action to which the term polarity has been applied, was manifested in a clear and conclusive manner. Still I thought it would contribute to the final settlement of the question if I were to take up the subject after the method of Weber, and satisfy all the demands which had been made upon him by the opponents of diamagnetic polarity. Here, as in the foregoing inquiry, it was my wish to render the experiments exhaustive, and to employ apparatus which should place it definitely within the power of all investigators to subject the question to experimental demonstration. I devised a scheme of experiment, but, previous to putting it into execution, wrote to Prof. Weber asking him whether he did not think it possible so to improve his apparatus as materially to exalt the action. Weber's own experiments had been made with bismuth solely. It was objected that his results were due to ordinary induced currents, and he was called upon to produce the same effects with insulators. This demand it was my object to meet, and to do so more delicate and more powerful means than any previously employed were necessary.

Professor Weber replied to me immediately, stating that the employment of an astatic system of magnets would be a great improvement; and he had the exceeding kindness to devise in detail the apparatus for me. It was admirably constructed under Prof. Weber's own superintendence by Leyser of Leipzig, and with it the following inquiry was conducted.—J. T., 1870.]

M. Weber's experiments; for though admirably suited to the exhibition of certain effects of ordinary induction, Mr. Faraday himself has shown how unsuitable the apparatus employed would be for the investigation of the question of diamagnetic polarity. See Experimental Researches (2653, 2654), vol. iii. p. 143.—J. T., May 9, 1855.

FIFTH MEMOIR.

FURTHER RESEARCHES ON THE POLARITY OF THE DIAMAGNETIC FORCE.*

Introduction.

A YEAR ago I placed before the Royal Society the results of an investigation 'On the Nature of the Force by which Bodies are repelled from the Poles of a Magnet.'† The simultaneous exhibition of attraction and repulsion in the case of magnetised iron is the fact on which the idea of the polarity of this substance is founded; and it resulted from the investigation referred to, that a corresponding duality of action was manifested by bismuth. In those experiments the bismuth was the moveable object upon which fixed magnets were caused to act, and from the deflection of the bismuth its polarity was inferred. But, inasmuch as the action is reciprocal, we ought also to obtain evidence of diamagnetic polarity by reversing the conditions of experiment; by making the magnet the moveable object, and inferring from its deflection the polarity of the mass which produces the deflection. This experiment would be complementary to those described in the communication referred to, and existing circumstances invested the experiment with a great degree of interest and importance.

In fact, an experiment similar to that here indicated was made by Professor W. Weber, previous to my investigation, and the result was such as to satisfy its author of the reverse polarity of diamagnetic bodies. I will not here enter into a minute description of the instrument and mode of experiment by which

* From the Philosophical Transactions for 1856, part i.; having been received by the Royal Society November 27, 1855, and read December 20, 1855.

† Philosophical Transactions, 1855; and Phil. Mag. for September 1855.

this result was obtained; for the instrument made use of in the present inquiry being simply a refinement of that made use of by M. Weber, its explanation will embrace the explanation of his apparatus. For the general comprehension of the criticisms to which M. Weber's results have been subjected, it is necessary, however, to remark, that in his experiments a bismuth bar, within a vertical spiral of copper wire, through which an electric current was transmitted, was caused to act upon a steel magnet freely suspended outside the spiral. When the two ends of the bar of bismuth were permitted to act successively upon the suspended magnet, a motion of the latter was observed, which indicated that the bismuth bar was polar, and that its polarity was the reverse of that of iron.

Notwithstanding the acknowledged eminence of M. Weber as an experimenter, this result failed to produce general conviction. Mr. Faraday, in his paper 'On the polar or other condition of diamagnetic bodies,'* had shown that results quite similar to those obtained by M. Weber, in his first investigation with bismuth, were obtained in a greatly exalted degree with gold, silver, and copper; the effect being one of induced currents and not of diamagnetic polarity. He by no means asserted that his results had the same origin as those obtained by M. Weber; but as the latter philosopher had made no mention of the source of error which Mr. Faraday's experiments rendered manifest, it was natural to suppose that it had been overlooked, and the observed action attributed to a wrong cause. In an article published in his 'Massbestimmungen' in 1852, M. Weber, however, with reference to this point, writes as follows:—'I will remark that the article transferred from the Reports of the Society of Sciences of Saxony to Poggendorff's *Annalen* was only a preliminary notice of my investigation, the special discussion of which was reserved for a subsequent communication. It will be sufficient to state here, that in the experiments referred to I sought to eliminate the inductive action by suitable combinations; but it is certainly far better to set aside this action altogether, as has been done in the experiments described in the present memoir.'

* Experimental Researches, 2640, Philosophical Transactions, 1850, p. 171.

One conviction grew and strengthened throughout these discussions—this, namely, that in experiments on diamagnetic polarity great caution is required to separate the pure effects of diamagnetism from those of ordinary induced currents. With reference to even the most recent experiments of M. Weber, referred to at the conclusion of the citation just made, it is strongly urged that there is no assurance that the separation referred to has been effected. In those experiments, as already stated, a cylinder of bismuth was suspended within a vertical helix of covered copper wire, and the action of the cylinder upon a magnet *suspended opposite to the centre or neutral point of the helix* was observed. To increase the action, the position of the cylinder was changed at each termination of the minute swing of the magnet, the amplitude of the oscillations being thus increased, and the effect rendered more sensible to the eye. Now, it is urged, there is every reason to believe that in these motions of a metallic mass within an excited helix induced currents will be developed, which, acting upon the magnet, will produce the motions observed. The failure indeed to demonstrate the existence of diamagnetic polarity by other means has, in the case of some investigators, converted this belief into a certainty.

Among the number whom M. Weber's experiments have failed to convince, M. Matteucci occupies a prominent place. With reference to the question before us, this philosopher writes as follows:—*

'In reading the description of the experiments of M. Weber, we are struck on beholding the effects produced by moving the bismuth when there is no current in the spiral. Although the direction of oscillation in this latter case is opposed to that observed when the spiral is active, still the fact excites doubts as to the truth of the conclusions which have been drawn from these experiments.† *To deduce rigorously the demonstration of diamagnetic polarity, it would be necessary to substitute for the*

* *Cours spécial sur l'Induction*, p. 206.

† It is not my place to account for the effect here referred to. I may, however, remark, that there appears to be no difficulty in referring it to the ordinary action of a diamagnetic body upon a magnet. It is the result which Brugmans published upwards of half a century ago; the peculiar form of this result in one of the series of experiments quoted by M. Weber must, I think, be regarded as purely accidental. —J. T.

massive bismuth, cylinders formed of insulated particles of the metal, to vary the dimensions of the cylinder, and above all, to compare the effects thus obtained with those which would probably be obtained with cylinders of copper and silver in a state of purity.*

'We are obliged,' continues M. Matteucci, 'to make the same remarks on another series of experiments which this physicist has made to obtain anew, by the effects of induction, the proof of diamagnetic polarity. It is astonishing, that after having sought to neutralise the development of induced currents in the moving cylinders of bismuth, by means of a very ingenious disposition of the spiral—it is astonishing, I repeat, that no attempt was made to prove by preliminary essays with metals possessing a higher conductibility than bismuth, that the same end could be obtained. I cannot leave you [M. Matteucci is here addressing his pupils] ignorant that the doubts which I have ventured to advance against the experiments of M. Weber are supported by the negative result which I have obtained in endeavouring to excite diamagnetic polarity in bismuth by the discharge of the Leyden jar.'

It will be seen in the following pages that the conditions laid down by M. Matteucci for the rigorous demonstration of diamagnetic polarity are more than fulfilled.

The conclusions of M. Weber find a still more strenuous opponent in his countryman Professor v. Feilitzsch, who has repeated Weber's experiments, obtained his results, but who denies the validity of his inferences. M. v. Feilitzsch argues, that in the experiments referred to it is impossible to shut out ordinary induction, and for the rigorous proof of diamagnetic polarity proposes the following conditions.† 'To render the experiment free from the action of induced currents two ways are open. The currents can be so guided that they shall mutually neutralise each other's action upon the magnet, or the induced currents can be *completely got rid of* by using, instead of a diamagnetic *conductor*, a diamagnetic *insulator*.' To test the question, M. v. Feilitzsch resorted to the latter method: instead of cylinders of bismuth he made use of cylinders of

* Also in page 204:—'Il fallait donc, pour prouver si l'influence d'un corps diamagnétique produit sur un aimant une variation de sens contraire à celle développée dans le fer doux, opérer avec ce corps *privé de conductibilité.*'

† Poggendorff's *Annalen*, xcii. 377.

wax, and also employed a prism of heavy glass, but in neither case was he able to detect the slightest action upon the magnet. 'However the motions of the prism might be varied, it was not possible either to cause the motionless magnet to oscillate, or to bring the magnet from a state of oscillation to one of rest.' M. v. Feilitzsch pushes his experiments further, and finds that when the bismuth is *motionless* within its spiral, the position of the magnet is just the same as when the bismuth is entirely withdrawn; hence his final conclusion, that the deflection of the magnet in Weber's experiments is due to induced currents, which are excited in the bismuth by its mechanical motion up and down within the spiral.

These divergent opinions upon a question of such vital bearing upon the general theory of magnetic phenomena, naturally excited in me the desire to make myself acquainted with the exact value of M. Weber's experiments. The most direct way of accomplishing this I considered to be, to operate with an instrument similar to that made use of by Weber himself; I therefore resolved to write to the constructor of his apparatus, but previous to doing so I wrote to M. Weber, inquiring whether his further reflections on the subject had suggested to him any desirable modification of his instrument. In reply to my question he undertook to devise for me an apparatus, surpassing in delicacy any hitherto made use of. The design of M. Weber was ably carried out by M. Leyser of Leipzig; and with the instrument thus placed in my possession, I have been able to satisfy the severest conditions proposed by those who saw in the results of Weber's experiments the effects of ordinary induction.

Description of Apparatus.

A sketch of the instrument employed in the present investigation is given in fig. 2. BO, B′O′ is the outline of a rectangular box, the front of which is removed so as to show the apparatus within. The back of the box is prolonged, and terminates in two semicircular projections, which have apertures at H and H′. Stout bolts of brass, which have been made fast in solid masonry, pass through these apertures, and the instrument, being secured to the bolts by screws and washers, is supported in a vertical position, being free from all disturbance save such as affects the

foundations of the Royal Institution. All the arrangements presented to the eye in fig. 2 are made fast to the back of the box, but are unconnected with the front, so as to permit of the removal of the latter. WW′ are two boxwood wheels with grooved peripheries, which permit of motion being transferred

Fig. 1.

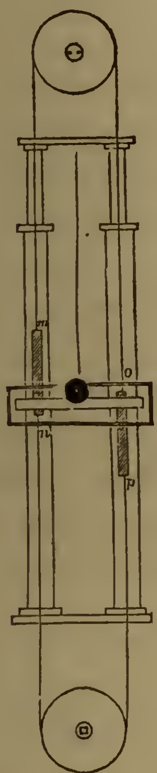


Fig. 2.

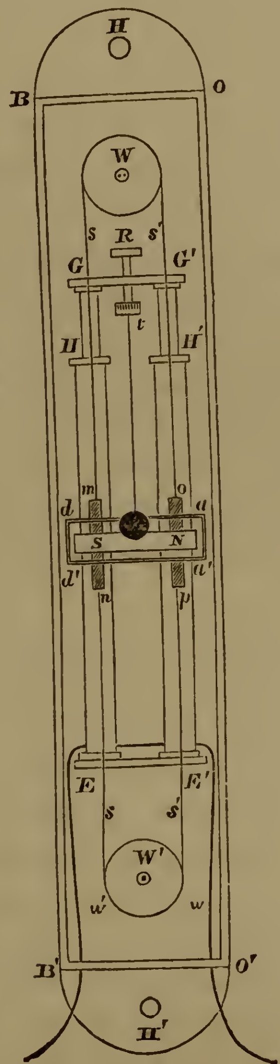


Fig. 3.

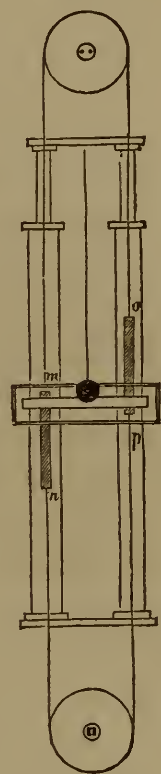
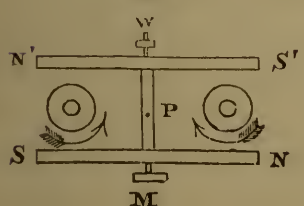


Fig. 4.



from one wheel to the other by means of a string *ss′*. Attached to this string are two cylinders, *mn*, *op*, of the body to be examined: in some cases the cylinders are perforated longitudinally, the string passes through the perforation, and the cylinders are supported by knots on the string. HE, H′E′ are two helices of copper wire overspun with silk, and wound round two brass reels, the upper ends of which protrude from H to G, and from H′ to G′. The internal diameter of each helix is 0·8

of an inch, and its external diameter about 1·3 inch; the length from H to E is 19 inches, and the centres of the helices are 4 inches apart; the diameters of the wheels WW′ being also 4 inches. The cross bar GG′ is of brass, and through its centre passes the screw R. From this screw depend a number of silk fibres which support an astatic arrangement of two magnets, the front one of which, SN, is shown in the figure. An enlarged section of the instrument through the astatic system is shown in fig. 4. The magnets are connected by a brass cross-piece, in which is the point of suspension P, fig. 4; and the position of the helices is shown to be between the magnets. It will be seen that the astatic system is a horizontal one, and not vertical, as in the ordinary galvanometer. The black circle in front of the magnet SN, fig. 2, is a mirror, which is shown in section at M, fig. 4; to balance the weight of this mirror, and adjust the magnets in a horizontal position, a brass washer, W, is caused to move along a screw, until a point is attained at which its weight brings both the magnets into the same horizontal plane. There is also another adjustment, which permits of the magnets being brought closer together or separated more widely asunder.

The motions of this compound magnet are observed by means of a distant scale and telescope, according to the method applied to the magnetometer of Gauss. The rectangle *da*, *d′a′*, fig. 2, is the section of a copper damper, which, owing to the electric currents induced in it by the motion of the magnet, soon brings the latter to rest, and thus expedites experiment.

It is well known that one end of a magnet attracts, while the other end repels the same pole of a magnetic needle; and that between both there is a neutral point which neither attracts nor repels. The same is the case with the helices HE, H′E′; so that when a current is sent through them, if the astatic magnet be exactly opposite the neutral point, it is unaffected by the helices. This is scarcely attainable in practice; a slight residual action remains which draws the magnets against the helices; but this is very easily neutralised by disposing an external portion of the circuit so as to act upon the magnets in a direction opposed to that of the residual action. Here then we have a pair of spirals which, when excited, do not act upon the magnets, and which therefore permit us to examine the pure

action of any body capable of magnetic excitement and placed within them.

In the experiments to be described, it was arranged that the current should always flow in opposite directions through the two spirals; so that if the cylinders within them were polar, the two upper ends of these cylinders should be poles of opposite names, and consequently the two lower ends opposed also. Suppose the two cylinders *mn op* to occupy the central position indicated in fig. 2: then, even if the cylinders became polar through the action of the surrounding current, the astatic magnets, being opposite to the neutral points of the cylinders, would experience no action from the latter. But suppose the wheel W′ to be so turned that the two cylinders are brought into the position shown in fig. 1, the upper end *o* of *op* and the lower end *n* of *mn* will act simultaneously upon the suspended magnets. For the sake of illustration, let us suppose the ends *o* and *n* to be both north poles, and that the section, fig. 4, is taken when the bars are in the position shown in fig. 1. The right-hand pole *o* will attract S′ and repel N, which attraction and repulsion sum themselves together to produce a deflection of the system of magnets. On the other hand, the left-hand pole *n*, being also north, will attract S and repel N′, which two effects also sum themselves to produce a deflection in the same direction as the former two. Hence, not only is the action of terrestrial magnetism annulled by this arrangement, but the moving force due to the reciprocal action of the magnets and the bodies within the helices is increased fourfold. By turning the wheel in the other direction, we bring the cylinders into the position shown in fig. 3, and thus may study the action of the ends *m* and *p* upon the magnets.

The screw R is employed to raise or lower the magnets. At the end, *t*, of the screw is a small torsion circle which can be turned independently; by means of the latter the suspending fibre can be twisted or untwisted without altering the level of the magnets.

The front is attached to the box by brass hasps, and opposite to the mirror M a small plate of glass is introduced, through which the mirror is observed; the magnets within the box being thus effectually protected from the disturbances of the external air. A small handle to turn the wheel W′ accompanied

the instrument from its maker; but in the experiments, I used, instead of it, a key attached to the end of a rod 10 feet long; with this rod in my right hand, and the telescope and scale before me, the experiments were completely under my own control. Finally, the course of the current through the helices was as follows:—Proceeding from the platinum pole of the battery it entered the box along the wire *w*, fig. 2, which passed through the bottom of the latter; thence through the helix to H′, returning to E′; thence to the second helix, returning to E, from which it passed along the wire *w*′ to the zinc pole of the battery. A commutator was introduced in the circuit, so that the direction of the current could be reversed at pleasure.

Experiments.—Deportment of Diamagnetic Bodies.

A pair of cylinders of chemically pure bismuth, 3 inches long and 0·7 of an inch in diameter, accompanied the instrument from Germany. These were first tested, commencing with a battery of one cell of Grove. Matters being as sketched in fig. 2, when the current circulated in the helices and the magnet had come to rest, the cross wire of the telescope cut the number 482 on the scale. Turning the wheel W′ so as to bring the cylinders into the position fig. 1, the magnet moved promptly, and after some oscillations took up a new position of equilibrium; the cross wire of the telescope then cut the figure 468 on the scale. Reversing the motion so as to place the cylinders again central, the former position 482 was assumed; and on turning further in the same direction, so as to place the cylinders as in fig. 3, the position of equilibrium of the magnet was at the number 493. Hence by bringing the two ends *n* and *o* to bear upon the astatic magnet, the motion was from greater to smaller numbers, the position of rest being then fourteen divisions less than when the bars were central. By bringing the ends *m* and *p* to bear upon the magnet, the motion was from smaller to greater numbers, the position of rest being eleven divisions more than when the bars were central.

As the positions here referred to will be the subject of frequent reference, for the sake of convenience I will call the position of the cylinders sketched in fig. 1, Position 1; that sketched in fig. 2, Position 2; and that sketched in fig. 3, Position 3. The

results which we have just described, tabulated with reference to these terms, would then stand thus:—

I.

Bismuth Cylinders.—Length 3 inches; diameter 0·7.

Position 1.	468	Position 2.	482	Position 3.	493.

In changing therefore from position 1 to position 3, a deflection corresponding to twenty-five divisions of the scale was produced.

Wishing to place myself beyond the possibility of illusion as regards the fact of deflection, I repeated the experiment with successive batteries of two, three and four cells. The following are the results:—

II.

	2 cells	3 cells	4 cells
Position 1.	450	439	425
Position 2.	462	450	437
Position 3.	473	462	448

In all the cases cited we observe the same result. From position 2 to position 1 the motion is from larger to smaller numbers; while from position 2 to position 3 the motion is from smaller to larger numbers.

It may at first sight appear strange that the amount of the deflection did not increase with the battery power; the reason, in part, is that the magnet, when the current circulated, was held in a position free from the spirals, by forces emanating partly from the latter and partly from a portion of the external circuit. When the current increased, the magnetisation of the bismuth increased also, but so did the force which held the magnets in their position of equilibrium. To remove them from this position, a greater amount of force was necessary than when only the residual action of a feeble current held them there. This fact, coupled with the circumstance that less heat was developed, and less disturbance caused by air currents, when a feeble battery was used, induced me for some time to experiment with a battery of two cells. Subsequent experience however enabled me to change this for five cells with advantage.

Notwithstanding the improbability of the argument, still it may be urged that these experiments do not prove beyond a doubt that the bismuth cylinders produce the motion of the

magnets in virtue of their excitement by the voltaic current; for it is not certain that these cylinders would not produce the same motion wholly independent of the current. Something of this kind has already occurred to M. Leyser,* and why not here?

In answer to this, I reply, that if the case be as here suggested, the motion of the magnet will not be changed when the current surrounding the bismuth cylinders flows in the opposite direction. Here is the experiment.

III.

Position 1. 764	Position 2. 742	Position 3. 704

We observe here that in passing from position 2 to position 1 the motion is from smaller to larger numbers; while in passing from position 2 to position 3 the motion is from larger to smaller numbers. This is the opposite result to that obtained when the current flowed in the opposite direction; and it proves *that the polarity of the bismuth cylinders depends upon the direction of the current, changing as the latter changes.* It was pleasant to observe the prompt and steady march of the magnet as the cylinders were shifted in the helices. When the magnets, operated on by the bars of bismuth, were moving in any direction, by bringing the two opposite ends of the bismuth cylinders into action, the motion could be promptly checked; the magnets could be brought to rest, or their movement converted into one in the opposite direction.

I may add to the above a series of results obtained some days subsequently in the presence of Professors Faraday, De la Rive, and Marcet.

IV.

Bismuth Cylinders.

Position 1. 670	Position 2. 650	Position 3. 630

The difference between positions 1 and 3 amounts here to forty divisions of the scale; subsequent experience enabled me to make it still greater.

It was found by experiment, that when the motion was from lower to higher numbers it denoted that the poles NN′, fig. 4, were repelled from the spirals, and the poles SS′ attracted towards them. When, on the contrary, the motion was from

* Scientific Memoirs, New Series, vol. i. page 184.

larger to smaller numbers, it indicated that the poles NN′ were attracted and the poles SS′ repelled. In the position fig. 1, therefore, of Tables III. and IV. the poles NN′ were repelled by the ends *n′o* of the bismuth cylinders, and the poles SS′ attracted; while in the position fig. 3, the poles NN′ were attracted by the ends *mp*, and the poles SS′ repelled; the ends *n* and *o*, therefore, acted as two north poles, while the ends *m* and *p* acted as two south poles. Now the direction of the current in the experiments recorded in the two tables referred to was that shown by the arrows in fig. 4. Standing in front of the instrument, the direction in the adjacent face of the spiral H′E′ was from right to left, while it was from left to right in HE. Hence, the polarity of the bismuth cylinders was the reverse of that which would be excited in cylinders of iron under the same circumstances. This assertion, however, shall be transferred from the domain of deduction to that of fact before we conclude.

Let us now urge against these experiments all that ever has been urged by the opponents of diamagnetic polarity. The bismuth cylinders are metallic conductors, and in moving them through the spirals induced currents more or less powerful will be excited in these conductors. The motion observed may not, after all, be due to diamagnetic polarity, but to the currents thus excited. I reply, that in all cases the number set down marks the *permanent* position of rest of the magnet. Were the action due to induced currents, these, being momentary, could only impart a *shock* to the magnet, which, on the disappearance of the currents, would return to its original position. But the deflection is permanent, and is therefore due to an enduring cause. In his paper on ‘Supposed Diamagnetic Polarity,’ Mr. Faraday rightly observes,—‘If the polarity exists, it must be in the particles, and for the time permanent, and therefore distinguishable from the momentary polarity of the mass due to induced temporary currents, and it must also be distinguishable from ordinary magnetic polarity by its contrary direction.’ These are the precise characteristics of the force made manifest by the experiments now under consideration.

Further, the strength of induced currents depends on the conducting power for electricity of the mass in which they are formed. Expressing the conducting power of bismuth by the

number 1·8, that of copper would be expressed by 73·6,* the conductivity of the latter being therefore forty times that of the former. Hence arises the demand made by the opponents of diamagnetic polarity, to have the experiments repeated with cylinders of copper; for if the effect be due to induced currents, they will show themselves in copper in a greatly increased degree. The following is the result of a series of experiments made with two copper cylinders, of the same dimensions as the bismuth ones already described:—

V.

Cylinders of Copper.

Position 1. 754	Position 2. 754	Position 3. 755

If the effects obtained with bismuth were due to induced currents, we ought to have the same effects forty times multiplied in the case of copper, in place of which we have scarcely any sensible effect at all.

Bismuth is the only substance which has hitherto produced a sensible action in experiments of this nature; another illustration, however, is furnished by the metal antimony, which possesses a greater conductive power, but a less diamagnetic power than bismuth. The following results were obtained with this substance:—

VI.

Cylinders of Antimony.—Length 3 inches; diameter 0·7.

	Current direct.†	Current reversed.‡
Position 1.	693	244
Position 2.	688	252
Position 3.	683	261

On comparing these numbers with those already obtained with bismuth, we observe that for like positions the actions of both metals are alike in direction. We further observe that the results are determined, not by the relative *conducting* powers of the two metals, but by their relative *diamagnetic* powers. If the former were the determining cause, we should have greater deflections with antimony than with bismuth, which is not the case; if the latter, we should have less deflections, which is the case.

The third and severest condition proposed by those who object to the experiments of M. Weber is to substitute insula-

* Philosophical Magazine, Series 4, vol. vii. p. 37.

† As in III. and IV.

‡ As in I. and II.

tors for conductors. I call this condition severe for the following reasons:—according to the experiments of Faraday,* when bismuth and sulphur are submitted to the same magnetising force, the repulsion of the former being expressed by the number 1968, that of the latter is expressed by 118. Hence an action which, with the means hitherto employed, was difficult of detection in the case of bismuth, must wholly escape observation in the case of sulphur. The same remarks apply, in a great measure, to all other insulators.

But the admirable apparatus made use of in this investigation has enabled me to satisfy this condition also. To Mr. Faraday I am indebted for the loan of two prisms of the selfsame heavy glass with which he made the discovery of diamagnetism. The bismuth cylinders were withdrawn from the helices and the prisms of glass put in their places. It was now necessary to have a perfectly steady magnet, the expected result being so small as to be readily masked by, or confounded with, a motion arising from some extraneous disturbance. The feeble warmth developed in the helices by an electric current from two cells was found able to create air currents of sufficient power to defeat all attempts to obtain the pure action of the prisms. To break up these air currents I stuffed all unfilled spaces of the box with old newspapers, and found the expedient to answer perfectly. With a fresh battery, which delivered a constant current throughout the duration of an experiment, the magnet was admirably steady,† and under these favourable conditions the following results were obtained:—

VII.

Prisms of Heavy Glass.—Length 3 inches; width 0·6; depth 0·5.

Current direct.	Current direct.	Current direct.
Position 1. 664	Position 2. 662	Position 3. 660

Thus in passing from position 1 to 3, or *vice versâ*, a permanent deflection corresponding to four divisions of the scale was produced. By raising or lowering the respective prisms at the proper moments the amplitude of the oscillations could be considerably augmented, and when at a maximum, could be speedily

* Phil. Mag. March 1853, p. 222.

† It was necessary, however, to select a portion of the day when Albemarle Street was free from cabs and carriages, as the shaking of the entire building, by the rolling of these vehicles, rendered the magnets unsteady.

extinguished by reversing the motions of the prisms. In six different series of experiments made with this substance the same invariable result was obtained. It will be observed that the deflections are in all cases identical in direction with those produced by bismuth under the same circumstances.

The following results were afterwards obtained with the same prisms in the presence of M. de la Rive; the current was 'direct.'

VIII.

Position 1.	652	Position 2.	650	Position 3.	648

On the negative result arrived at with this substance, it will be remembered that M. v. Feilitzsch bases one of his arguments against the conclusions of M. Weber.

Calcareous spar was next submitted to experiment. Two cylinders of the transparent crystal were prepared and examined in the manner already described. The results are as follows:—

IX.

Cylinders of Calcareous Spar.—Length 3 inches; diameter 0·7.

	Current direct.		Current direct.		Current direct.
Position 1.	699·5	Position 2.	698·5	Position 3.	697·5

Here, as in the other cases, the deflection was permanent, and could be augmented by the suitable raising or lowering of the respective cylinders. The action is small, but perfectly certain. The magnet was steady and moved promptly and invariably in the directions indicated by the numbers. It will be observed that the deflections are the same in kind as those produced by bismuth.

The intrusion of other employments compelled me to postpone the continuation of these experiments for several weeks. On taking up the subject again, my first care was to assure myself that the instrument retained its sensibility. Subsequent to the experiments last recorded it had been transported over several hundred miles of railway, and hence the possibility of a disturbance of its power. The following experiments, while they corroborate the former ones, show that the instrument retained its power and delicacy unimpaired:—

X.

Bismuth Cylinders.

	Current direct.	Current reversed.
Position 1.	612	264
Position 2.	572	230
Position 3.	526	200

The deflections, it will be observed, are the same in kind as before; but by improved manipulation the effect is augmented. In passing from position 1 to 3 we have here a deflection amounting in one case to 64, and in the other to 86 divisions of the scale.

To Mr. Noble I am indebted for two cylinders of pure statuary marble; the examination of these gave the following results:—

XI.

Cylinders of Statuary Marble.—Length 4 inches; diameter 0·7.

	Current direct.	Current reversed.
Position 1.	601	215
Position 2.	598	218
Position 3.	596	220

Here, in passing from position 1 to 3, we have a permanent deflection corresponding to five divisions of the scale. As in all other cases, the impulsion of the magnet might be augmented by changing the position of the cylinders at the limit of each swing. The deflections are the same in kind as those produced by bismuth, which ought to be the case, for marble is diamagnetic.

An upright iron stove influenced by the earth's magnetism becomes a magnet with its bottom a north and its top a south pole. Doubtless, though in an immensely feebler degree, every erect marble statue is a true diamagnet, with its head a north pole and its feet a south pole. The same is certainly true of a man as he stands upon the earth's surface, for all the tissues of the human body are diamagnetic.

A pair of cylinders of phosphorus enclosed in thin glass tubes were next examined.

XII.

Cylinders of Phosphorus.—Length 3·5 inches; diameter 0·63.

	Current direct.		Current reversed.
	Series I.	Series II.	
Position 1.	620	670	224
Position 2.	618	668	226
Position 3.	616	666	228

The change of the bars from position 1 to 3 is in this case accompanied by permanent deflection corresponding to four divisions of the scale. The deflection and polarity is that of a diamagnetic body. The magnet was remarkably steady during these experiments, and the consequent clearness and sharpness of the result pleasant to observe.

XIII.

Cylinders of Sulphur.—Length 6 inches; diameter 0·7.

	Current direct.	Current reversed.
Position 1.	658·5	222
Position 2.	657	223·5
Position 3.	655·5	225·5

XIV.

Cylinders of Nitre.—Length 3·5 inches; diameter 0·7.

	Current direct.	Current reversed.
Position 1.	648·5	263
Position 3.	647	265

Finally, as regards solid diamagnetic bodies, a series of experiments was made with wax; this also being one of the substances whose negative deportment is urged by M. v. Feilitzsch against M. Weber.

XV.

Cylinders of Wax.—Length 4 inches; diameter 0·7.

	Current direct.	Current reversed.
Position 1.	624·5	240
Position 3.	623	241

The action is very small, but it is nevertheless perfectly certain, and proves the polarity of the wax. The argument founded on the negative deportment of this substance must therefore give way. When we consider the feebleness of the action with so delicate a means of examination, the failure of M. v. Feilitzsch to obtain the effect, with an instrument constructed by himself, will not excite surprise.

Thus in the case of seven insulating bodies the existence of diamagnetic polarity has been proved. The list might be augmented without difficulty; but sufficient I trust has been done to remove the scruples of those who saw in M. Weber's results an action produced by induced currents.

Polarity of Diamagnetic Liquids.

A portion of the subject hitherto untouched, but one of great interest, has reference to the polar condition of liquid bodies while under magnetic influence.

The first liquid examined was distilled water; it was enclosed in thin glass tubes, corked at the ends; and by means of a loop passing round the cork, the tubes were attached to the string passing round the wheels WW′. Previous to use, the corks

were carefully cleansed, so that any impurity contracted in cutting, or by contact with ferruginous matters, was completely removed. The following are the results obtained with this liquid :—

XVI.

Cylinders of Distilled Water.—Length 4 inches; diameter 0·65.

	Current direct.	Current reversed.
Position 1.	605	246
Position 2.	603	248
Position 3.	601	250

The experiment was many times repeated, but always with the same result; indeed, the polarity of the water is as safely established as that of iron. Pure water is diamagnetic, and the deflections produced by it are the same as those of all the other diamagnetic bodies submitted to examination.

From the position which it occupies in Mr. Faraday's list,* I had also some hopes of proving the polarity of sulphide of carbon. The following results were obtained :—

XVII.

Cylinders of Bisulphide of Carbon.—Length 4 inches; diameter 0·65.

	Current direct.	Current reversed.
Position 1.	631	210
Position 2.	629	213
Position 3.	626	216

As in the case of distilled water, we observe a deflection in one direction when the current is 'direct,' and in the other when it is 'reversed,' the action in the first case, in passing from position 1 to 3, amounting to five, and in the latter case to six divisions of the scale. The polarity of the substance is therefore established, and it is that of diamagnetic bodies.

Deportment of Magnetic Bodies.

Thus far we have confined our examination to diamagnetic substances: turn we now to the deportment of magnetic bodies when submitted to the same conditions of experiment. Here we must select substances suitable for examination, for all are not so. Cylinders of iron, for example, of the same size as our diamagnetic cylinders, would, through the intensity of their action, quite derange the apparatus; so that we are obliged to

* Phil. Mag. March 1853, p. 222.

have recourse to bodies of smaller size or of feebler magnetic capacity. Besides, the remarks of writers on this subject render it of importance to examine whether bodies through which the magnetic constituents are very sparingly distributed present a veritable polarity the same as that exhibited by iron itself.

Slate rock usually contains from eight to ten per cent. of oxide of iron, and a fragment of the substance presented to the single pole of an electro-magnet is attracted by the pole. A cylinder of slate from the Penrhyn quarries near Bangor was first examined. It was not found necessary to increase the effect by using two cylinders, and the single one used was suspended in the right-hand helix H′E′. The deportment of the substance was as follows:—

XVIII.

Cylinder of Penrhyn Slate.—Length 4 inches; diameter 0·7.

	Current direct.	Current reversed.
Position 1.	620	280
Position 2.	647	240
Position 3.	667	193

Comparing these deflections with those obtained with diamagnetic bodies, we see that they are in the opposite direction. With the direct current a change from position 1 to 3 is followed, in the case of diamagnetic bodies, by a motion from higher to lower numbers; while in the present instance the motion is from lower numbers to higher. In the former case the north poles of the astatic magnet are attracted, in the latter they are repelled. We also see that a *direct* current acting on diamagnetic bodies produces the same deflection as a *reverse* current on magnetic ones. Thus, as promised at page 165, the opposite polarities of diamagnetic and magnetic bodies are transferred from the region of deduction to that of fact.

XIX.

Cylinder of Caermarthen Slate.—Length 4 inches; diameter 0·7.

	Current direct.	Current reversed.
Position 1.	664	300
Position 2.	690	235
Position 3.	720	185

The deflections in this case are also indicative of magnetic polarity.

These two cylinders were so taken from the rock that the axis of each lay in the plane of cleavage. The following experiments, made with a cylinder of the same size, show the capa-

bility of a rock of this structure to be magnetised across the planes of cleavage.

XX.

Cylinder of Slate: axis of cylinder perpendicular to cleavage.

	Current direct.	Current reversed.
Position 1.	655	240
Position 2.	678	205
Position 3.	695	192

Chloride of iron was next examined: the substance, in powder, was enclosed in a single glass tube, which was attached to the string passing round the wheels WW′ of the instrument.

XXI.

Cylinder of powdered Chloride of Iron.—Length 3·8 inches; diameter 0·5.

	Current direct.	Current reversed.
Position 1.	185	990
Position 2.	—	230
Position 3.	990	185

The deflection here indicates magnetic polarity. The action was very powerful. When swiftly moving in any direction, a change in the position of the cylinder instantly checked the magnet in its course, brought it to rest, or drove it forcibly in the opposite direction. The numbers 185 and 990 mark indeed the utmost limit between which it was possible for the magnet to move; here it rested against the helices.

Two glass tubes were filled with red oxide of iron and examined. The action of the poles of these cylinders upon the magnets was so strong, as to efface, by the velocity imparted to the magnets, all distinct impression of the numbers on the scale. By changing the position of the tubes within the helices, the magnets could be driven violently through the field of view, or could be held rigidly against the respective helices. As in all other cases, the centres of the cylinders were neutral points, and the two ends of each were poles of opposite qualities. The polarity was the same as that of iron.

A small quantity of iron filings was kneaded thoroughly in wax, and a cylinder formed from the mass. Its deportment was also very violent, and its polarity was just as clear and pronounced as that of a solid cylinder of iron could possibly be.

Sulphate of iron was next examined: the crystallised substance was enclosed in two glass tubes and tested in the usual manner.

XXII.

Cylinders of Sulphate of Iron.—Length 4·5 inches; diameter 0·7.

	Current direct.	Current reversed.
Position 1.	510	510
Position 2.	600	370
Position 3.	700	220

The red ferroprussiate of potassa is a magnetic salt; with this substance the following results were obtained:—

XXIII.

Cylinders of red Ferroprussiate of Potassa.—Length 4·5 inches; diameter 0·65.

	Current direct.	Current reversed.
Position 1.	610	250
Position 2.	630	220
Position 3.	655	197

In this case also the crystallised salt was enclosed in glass tubes.

Two glass tubes were next filled with carbonate of iron in the state of powder: the following are the results:—

XXIV.

Cylinders of Carbonate of Iron.—Length 4 inches; diameter 0·5.

Current direct.	Current direct	Current direct.
Position 1. 185	Position 2. 620	Position 3. 740

In all these cases the deflections show that the cylinders of powder are true magnets, being polar after the manner of iron.

Polarity of Magnetic Liquids.

As the complement of the experiments made with diamagnetic liquids, we now pass on to the examination of the polarity of magnetic liquids. A concentrated solution of sulphate of iron was enclosed in two glass tubes and submitted to examination.

XXV.

Sulphate of Iron Solution in tubes.—Length 4 inches; diameter 0·65.

Current direct.	Current direct.	Current direct.
Position 1. 548	Position 2. 600	Position 3. 648

A solution of muriate of nickel, examined in the same manner, gave the following results:—

XXVI.

Muriate of Nickel Solution in tubes.—Length 3·6 inches; diameter 0·65.

	Current direct.	Current reversed.
Position 1.	605	224
Position 2.	632	200
Position 3.	650	185

A solution of muriate of cobalt yielded as follows :—

XXVII.

Muriate of Cobalt solution in tubes.—Length 3·6 inches ; diameter 0·65.

	Current direct.	Current reversed.
Position 1.	630	262
Position 2.	645	235
Position 3.	660	202

In all these cases we have ample evidence of a polar action the reverse of that exhibited by diamagnetic liquids. These are the first experiments in which the action of either liquid magnets or liquid diamagnets upon a suspended steel magnet has been exhibited.

Thus far then the following substances have been submitted to examination :—

Diamagnetic bodies.	Magnetic bodies.
Bismuth.	Penrhyn slate.
Antimony.	Slate, axis parallel to cleavage.
Heavy glass.	Slate, axis perpendicular to cleavage.
Calcareous spar.	Chloride of iron.
Statuary marble.	Sulphate of iron.
Phosphorus.	Carbonate of iron.
Sulphur.	Ferrocyanide of potassium.
Nitre.	Oxide of iron.
Wax.	Iron filings.
Liquids.	Liquids.
Distilled water.	Sulphate of iron.
Bisulphide of carbon.	Muriate of nickel.
	Muriate of cobalt.

Every substance in each of these lists has been proved to be polar under magnetic influence, the polarity of the diamagnetic bodies being invariably opposed to that of the magnetic ones.

In his investigation on the supposed polarity of diamagnetic bodies, Mr. Faraday made use of a core of sixpenny pieces, and obtained with it the results he sought. Wishing to add the testimony of silver as a good conductor to that of copper, two cylinders were formed of sixpenny pieces, covered with paper, and submitted to experiment. The following are the results obtained :—

XXVIII.

Silver cylinders (sixpenny pieces).

Current direct.	Current direct.	Current direct.
Position 1. 724	Position 2. 774	Position 3. 804

The action here was prompt and energetic, strongly contrasted with the neutrality of copper ; but the deflection was permanent,

and could not therefore be the result of induced currents. Further, it was a deflection which shows magnetic polarity, whereas pure silver is feebly diamagnetic. The cylinders were removed and examined between the poles of an electro-magnet; they proved to be magnetic.

On observing this deportment of the silver, I tried the copper cylinders once more. The results with a direct current were,—

XXIX.

Position 1. 766	Position 2. 767	Position 3. 768

Here almost the same neutrality as before is evidenced.

Deeming that the magnetism of the cores of silver coins was due to magnetic impurity attaching itself to the paper which covered them, a number of fourpenny pieces were procured, washed in ammonia and water, and enclosed in thin glass tubes. The following were the results :—

XXX.

Silver cylinders (fourpenny pieces).

Current direct.	Current direct.	Current direct.
Position 1. 490	Position 2. 565	Position 3. 660

Here also we have a very considerable action indicative of magnetic polarity. On examining the cylinders between the poles of an electro-magnet, they were found decidedly magnetic. This, therefore, appears to be the common character of our silver coins. [They doubtless contain a trace of iron]. The tubes which contained the pieces were sensibly neutral.

Knowing the difficulty of demonstrating the existence of diamagnetic polarity in ordinary insulators, M. Matteucci suggested that insulated fragments of bismuth ought to be employed, the insulation being effected by a coat of lac or resin. I constructed a pair of cylinders in accordance with the suggestion of M. Matteucci. The following are the results they yielded with a direct current :—

XXXI.

Position 1. 730	Position 2. 750	Position 3. 768

Here we have a very marked action, but the polarity indicated is magnetic polarity. On subsequent examination, the cylinders proved to be magnetic. This was due to impurities attaching themselves to the resin.

But the resin may be done away with and the powdered metal

still rendered an insulator. This thought was suggested to me by an experiment of Mr. Faraday, which I will here describe. Referring to certain effects obtained in his investigations on supposed diamagnetic polarity, he writes thus :—'If the effect were produced by induced currents in the mass, division of the mass would stop these currents and so alter the effect; whereas, if produced by *a true diamagnetic polarity*, division of the mass would not affect the polarity seriously or in its essential nature. Some copper filings were therefore digested for a few days in dilute sulphuric acid to remove any adhering iron, then well-washed and dried, and afterwards warmed and stirred in the air, until it was seen by the orange colour that a very thin film of oxide had formed upon them; they were finally introduced into a glass tube and employed as a core. It produced no effect whatever, but was as inactive as bismuth.' (Exper. Resear. 2658.)

Now when bismuth is powdered and exposed to the action of the air, it very soon becomes tarnished, even without heating. A quantity of such powder was prepared, and its conducting power for electricity tested. The clean ends of two copper wires proceeding from a battery of Grove were immersed in the powder; but though the wires were brought as near as possible to each other, short of contact, not the slightest action was observed upon a galvanometer placed in the circuit. When the wires touched, the needle of the galvanometer flew violently aside, thus proving that the current was ready, but that the powder was unable to conduct it. Two glass tubes were filled with the powder and submitted to experiment. The following results were obtained :—

XXXII.

Cylinders of Bismuth Powder.

Length 3 inches. Diameter 0·7.

	Current direct.	Current reversed.
Position 1.	640	230
Position 2.	625	245
Position 3.	596	260

These deflections are the same in kind as those obtained with the cylinders of massive bismuth. We have here no cessation of action. *The division of the mass does not affect the result seriously or in its essential nature, and hence the deportment exhibits the characteristics of 'a true diamagnetic polarity.'*

In summing up the results of his inquiry on this subject,

Mr. Faraday writes thus:—'Finally, I am obliged to say that I can find no experimental evidence to support the hypothetical view of diamagnetic polarity, either in my own experiments, or in the repetition of those of Weber, Reich, and others. . . . It appears to me also, that, as magnetic polarity conferred by iron or nickel in small quantity, and in unfavourable states, is far more easily indicated by its effects upon an astatic needle, or by pointing between the poles of a strong horseshoe magnet, than by any such arrangement as mine or Weber's or Reich's, so *diamagnetic polarity would be much more easily distinguished in the same way*.' I was struck, on reading this passage, to find how accurately the surmise has been fulfilled by the instrument with which the foregoing experiments were made. In illustration of the powers of this instrument, as compared with that made use of by Mr. Faraday, I may be permitted to quote the following result from his paper on supposed diamagnetic polarity so often referred to:—'A thin glass tube, 5½ inches by three-quarters of an inch, was filled with a saturated solution of protosulphate of iron, and employed as an experimental core; the velocity given to the machine at this and all average times was such as to cause five or six approaches and withdrawals of the core in one second; yet the solution produced no sensible indication on the galvanometer.' Referring to Table XXV., it will be seen that the instrument made use of in the present inquiry has given with a solution of protosulphate of iron a deflection amounting to no less than one hundred divisions of the scale. Mr. Faraday proceeds:—'A tube filled with small crystals of protosulphate of iron caused the needle to move about 2°. . . . . Red oxide of iron produced the least possible effect.' In the experiments recorded in the foregoing pages, the crystallised sulphate of iron gave a deflection of nearly two hundred divisions of the scale, while the red oxide gave a deflection as wide as the helices would permit, which corresponds to about eight hundred divisions of the scale. The correctness of Mr. Faraday's statement regarding the inferiority of the means first devised to investigate this subject, is thus strikingly illustrated. It might be added, that red ferroprussiate of potash and other substances, which have given me powerful effects, produced no sensible impression in experiments made with the other instrument.

Thus have we seen the objections raised against diamagnetic polarity fall away one by one, and a body of evidence accumulated in its favour, which places it among the most firmly established truths of science. This I cannot help thinking is mainly to be attributed to the bold and sincere questioning of the principle when it seemed questionable. The cause of science is more truly served, even by the denial of what may be a truth, than by the indolent acceptance of it on insufficient grounds. Such denials drive us to a deeper communion with Nature, and, as in the present instance, compel us through severe and laborious inquiry to strive after certainty, instead of resting satisfied, as we are prone to do, with mere probable conjecture.

Royal Institution, November 1855.

FARADAY'S LETTER TO MATTEUCCI ON DIAMAGNETIC POLARITY.

The following beautiful letter, extracted from Dr. Bence Jones's 'Life and Letters of Faraday,' shows the views of diamagnetic polarity entertained by Faraday in 1855. It was written prior to the publication of the foregoing memoir, but I have no reason to believe that the views here expressed were ever changed.

'November 2, 1855.

'My dear Matteucci,—When I received your last of October 23, I knew that Tyndall would return from the country in a day or two, and so waited until he came. I had before that told him of your desire to have a copy of his paper, and I think he said he would send it to you; I have always concluded he did so, and therefore thought it best to continue the same open practice and show him your last letter, note and all.

'As I expected, he expressed himself greatly obliged by your consideration, and I have no doubt will think on, and repeat, your form of experiment; but he wished you to have no difficulty on his account. I conclude he is quite assured in his own mind, but does not for a moment object to counter views, or to their publication; and I think feels a little annoyed that you should imagine for a moment that he would object to or be embarrassed by your publication. I think in that respect he is of my mind, that we are all liable to error, but that we love the truth, and speak only what at the time we think to be truth; and ought not to take offence when proved to be in error, since the error is not intentional; but be a little humbled and so turn the correction of the error to good account. I cannot help thinking that there are many apparent differences amongst us, which are not differences in reality. I differ from Tyndall a good deal in phrases, but when I talk with him I do not find

that we differ in facts. That phrase polarity in its present undefined state is a great mystifier.

'Well! I am content, and I suppose he is, to place our respective views before the world, and there leave them. Although often contradicted, I do not think it worth while reiterating the expressions once set forth or altering them, until I either see myself in the wrong or misrepresented, and even in the latter case I let many a misrepresentation pass. Time will do justice in all these cases.

'One of your letters asks me, what do you conceive the nature of the lines of magnetic force to be? I think it wise not to answer that question by an assumption, and therefore have no further account to give of such physical lines than that already given in my various papers. See that referred to already in the "Philosophical Magazine" (3301-3305); and I would ask you to read also 3299, the last paragraph in a paper in the "Philosophical Magazine," June 1852, which expresses truly my present state of mind.

'But a physical line of force may be dealt with experimentally without our knowing its intimate physical nature. A ray of light is a physical line of force; it can be proved to be such by experiments made whilst it was thought to be an emission, and also by other experiments made since it has been thought to be an undulation. Its physical character is not proved either by the one view or the other (one of which must be, and both may be wrong), but it is proved by the time it takes in propagation, and by its curvatures, inflections and physical affections. So with other physical lines of force, as the electric current; we know no more of the physical nature of the electric lines of force than we do of the magnetic lines of force; we fancy, and we form hypotheses, but unless these hypotheses are considered equally likely to be false as true, we had better not form them; and therefore I go with Newton when he speaks of the physical lines of gravitating force (3305 note), and leave that part of the subject for the consideration of my readers.

'The use of lines of magnetic force (without the physical) as true representations of nature, is to me delightful, and as yet never failing; and so long as I can read your facts, and those of Tyndall, Weber, and others by them, and find they all come into one harmonious whole, without any contradiction, I am

content to let the erroneous expressions, by which they seem to differ, pass unnoticed. It is only when a fact appears that they cannot represent that I feel urged to examination, though that has not yet happened. All Tyndall's results are to me simple consequences of the tendency of paramagnetic bodies to go from weaker to stronger places of action, and of diamagnetic bodies to go from stronger to weaker places of action, combined with the true polarity or direction of the lines of force in the places of action. . . . . . .

'These principles, or rather laws, explain to me all those movements obtained by Tyndall against which your note is directed, and therefore I do not see in his experiments any proofs of a defined or inverse polarity in bismuth, beyond what we had before. He has worked out well the antithetical relations of paramagnetic and diamagnetic bodies, and distinguished mixed actions which by some have been much confused; but the true nature of polarity, and whether it is the same or reversed in the two classes, is to my mind not touched. What a quantity I have written to you, all of which has no doubt been in your own mind, and tried by your judgment! Forgive me for intruding it.

'Ever truly yours,

'M. Faraday.'

The circumstances in which this letter originated are these. On the receipt of my paper, 'On the Nature of the Force by which Bodies are repelled from the Poles of a Magnet,' Matteucci undertook to repeat the experiments there recorded, but failed to obtain the results. He considered the memoir a tissue of error from beginning to end, and thought my character as a scientific man so gravely compromised that he wrote to ask Faraday for advice as to how he ought to act under the circumstances. Faraday showed me Matteucci's letter, and the result of our conversation regarding it is stated by Faraday himself. Weeks, it may have been months, elapsed without my hearing anything further about the matter; when at length a terse, frank letter reached me direct from Matteucci, the substance of which was this :—'I have written to Faraday, to Grove, and to Wheatstone, stating that your experiments were wrong. I now wish to give you the opportunity of cor-

recting me, and of saying to these gentlemen that I have repeated all your experiments and found them true to the letter.'

I think it probable that as regards diamagnetic polarity, Faraday and myself were looking at two different things. I limited my view to that doubleness of action in which the term polarity originated, and which causes electricity, as well as magnetism, to be regarded as a polar force. Faraday, I doubt not, had his mind fixed upon his lines of magnetic force. To this conception, however, though it formed the guiding light of his researches, he never gave a mechanical form. Hence arose his difficulty in dealing with the phenomena exhibited by crystals in the magnetic field. His thoughts doubtless dwelt in the profoundest depths of the subject. His great discovery of the rotation of the plane of polarisation had connected the force of magnetism with the luminiferous ether; and this future investigators will probably prove to be the domain of all magnetic action.* In the sense, however, in which the term polarity, as applied to magnetic phenomena, has been hitherto understood, the polarity of the diamagnetic force is, I think, conclusively demonstrated.

The efforts of Faraday to resolve magne-crystallic phenomena are mentioned in the Introduction to these papers. He says plainly that neither an attractive nor a repulsive force is competent to explain the effects which he discovers in such numbers and describes with such care. In the following short paper an attempt is made to solve by means of the doctrine of diamagnetic polarity the mechanical difficulties which beset Faraday's own mind. It will be seen that while he was perfectly right in his denial that either attraction or repulsion could produce the phenomena, a force compounded of both, in other words, a polar force, lies at the bottom of them all.—J. T., 1870.

* A conclusion to which the researches of Thomson and Maxwell even now distinctly point.

SIXTH MEMOIR.

ON THE RELATION OF DIAMAGNETIC POLARITY TO MAGNE-CRYSTALLIC ACTION.*

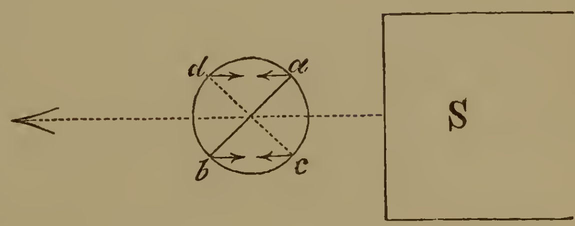
In a communication presented to the Royal Society some weeks ago, the fact of diamagnetic polarity was established in the case of insulators, among which phosphorus, sulphur, calcareous spar, statuary marble, heavy glass, nitre, and wax were comprised. The demonstration was also extended to distilled water and other liquids; and thus the conditions proposed by the opponents of diamagnetic polarity for its rigorous demonstration were fulfilled. The importance of the principle is demonstrated by the fruitfulness of its consequences; for by it we obtain a clear insight of effects which, without it, would remain standing enigmas in science, being connected by no known tie with the ordinary laws of mechanics. Many of the phenomena of magne-crystallic action are of this paradoxical character. For the sake of those who see no clear connection between these and the other effects of magnetism, as well as for the sake of completeness, I will here endeavour to indicate in a simple manner, and from my own point of view, the bearing of the question of polarity upon that of magne-crystallic action. I will commence with the elementary phenomena, and select for illustration as I proceed, cases of real difficulty which have been actually encountered by those who have worked experimentally at the subject.

To liberate the thoughts from all effects except those which are purely magne-crystallic, we will for the present operate with spheres. Let a sphere of carbonate of lime be suspended before the pole S, fig. 1, of an electro-magnet, so that the axis of the crystal shall be horizontal. Let the line *ab* mark any position of the axis inclined to the direction of the force emanating

* Phil. Mag. vol. ii. p. 125.

from S; and let the dotted line *dc* make an equal angle with the direction of the force at the other side. As the sphere is diamagnetic, the face of it which is turned towards S will, according to the principles established in the paper above referred to, be hostile to S, while that turned *from* S will be friendly to S; and, if the sphere were homogeneous, the tendency to set *ab* at right angles to the direction of the force would be exactly neutralised by the tendency to set *cd* in the same position: the sphere would consequently stand still. But the case is otherwise when the intensity of diamagnetisation along *ab* is greater than along *cd*, which I have elsewhere proved to be the fact.* If we suppose the sphere to vanish, with the exception of two thin needles taken along the lines mentioned, the hostile pole at *a* will be stronger than that at *c*, and the friendly pole at *b* will be stronger than that at *d*; hence the ends *a* and *b* *being acted upon by a mechanical couple of superior power*, the line *ab* will recede from its inclined position, and finally set itself *at right angles* to the direction of the force. Whatever be the inclination of the line *ab* to the magnetic axis, this superiority will belong to its couple; it is therefore manifest that the entire sphere will turn in the manner here indicated, and finally set with the axis of the crystal equatorial, which is the result established by experiment.

Fig. 1.



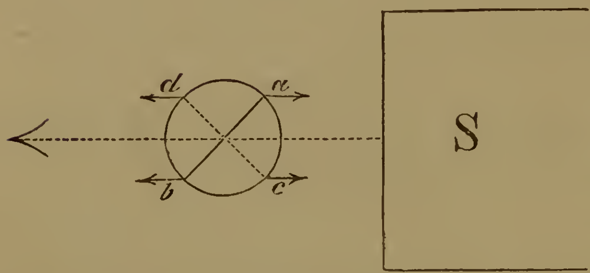
For the diamagnetic calcium, contained in this crystal, let the magnetic element, iron, be substituted. Each molecule of the crystal becomes thereby magnetic; we have carbonate of iron in place of carbonate of lime; and the line which, in the latter substance is that of maximum repulsion, is that of maximum attraction in the former. This, I think, is one of the most suggestive points † that researches in magne-crystallic action have hitherto established, namely, that the self-same arrange-

* Phil. Mag. S. 4. vol. ii. p. 176, and at pp. 50, 51 of this volume.

† For its bearing upon the question of a magnetic medium see Phil. Mag. vol. ix. p. 208, and this volume, p. 213.

ment of particles influences the paramagnetic and diamagnetic forces in the self-same way, intensifying both in the same direction. Let us suppose, then, that the sphere of carbonate of iron is suspended as in fig. 2, the line *ab* being the axis of the crystal. I have already shown that this line is that in which the magnetic induction is most intense.* Comparing, as before, the lines *ab* and *cd*, the friendly pole *a* is stronger than *c*, and the hostile pole *b* is stronger than *d*; a residual 'couple' therefore acts upon *ab* in the direction indicated by the arrows, which must finally set this line *parallel* to the direction of the force. This is also the result which experiment exhibits.

Fig. 2.



We will now proceed to apply the principle of polarity to some of the more complicated forms of magne-crystallic action. Some highly paradoxical effects were adduced by Mr. Faraday [in proof of the position that the magne-crystallic force is neither attraction nor repulsion]. I cannot bring the subject in a clearer manner before the reader than by quoting Faraday's own description of the phenomena referred to. Here it follows:—

'Another very striking series of proofs that the effect is not due to attraction or repulsion was obtained in the following manner. A skein of fifteen filaments of cocoon silk, about 14 inches long, was made fast above, and then a weight of an ounce or more hung to the lower end; the middle of this skein was about the middle of the magnetic field of the electro-magnet, and the square weight below rested against the side of a block of wood so as to give a steady silken vertical axis without swing or revolution. A small strip of card, about half an inch long and the tenth of an inch broad, was fastened across the middle of this axis by cement; and then a small prismatic crystal of sulphate of iron 0·3 of an inch long and 0·1 in thickness, was attached to the card, so that the length and also the magne-crystallic axis were in the horizontal plane; all the length was on one side of

* Phil. Mag. S. 4, vol. ii. p. 177, and at p. 52 of this volume.

the silken axis, so that as the crystal swung round, the length was radius to the circle described, and the magne-crystallic axis parallel to the tangent.

'When the crystal was made to stand between the flat-faced poles, the moment the magnet was excited it moved, tending to stand with its length equatorial, or its magne-crystallic axis parallel to the lines of force. When one pole was removed and the experiment repeated, the same effect took place, but not so strongly as before; finally, when the pole was brought as near to the crystal as it could be without touching it, the same result occurred, and with more strength than in the last case. In the two latter experiments, therefore, the crystal of sulphate of iron, though a magnetic body, and strongly attracted by such a magnet as that used, actually *receded* from the pole of the magnet under the influence of the magne-crystallic condition.

'If the pole S* be removed, and that marked N be retained for action on the crystal, then the latter approaches the pole urged by both the magnetic and magne-crystallic forces; but if the crystal be revolved 90° to the left, or 180° to the right, round the silken axis, so as to come into the contrary or opposite position, then this pole repels or rather causes the removal to a distance of the crystal, just as the former did. The experiment requires care, and I find that conical poles are not good; but with attention I could obtain the results with the utmost readiness.

'The sulphate of iron was then replaced by a crystalline plate of bismuth, placed, as before, on one side of the silk suspender, and with its magne-crystallic axis horizontal.† Making the position the same as that which the crystal had in relation to the N pole in the former experiment, so that to place its axis parallel to the lines of magnetic force it must approach this magnetic pole, and then throwing the magnet into an active state, the bismuth moved accordingly and did approach the pole, against

* The figures will be given and explained further on.

† It will be borne in mind that Faraday calls the line in a crystal which sets from pole to pole, the magne-crystallic axis of the crystal, whether the latter is paramagnetic or diamagnetic. In bodies of the former class, however, the 'axis' sets from pole to pole because the attraction along it is a maximum; while in bodies of the latter class, the 'axis' sets from pole to pole because the repulsion along the line perpendicular to it is a maximum.

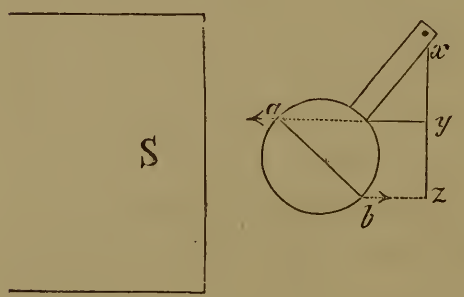
its diamagnetic tendency, but under the influence of the magne-crystallic force.

'Hence a proof that neither attraction nor repulsion governs the set. . . . . . This force, then, is distinct in its character and effects from the magnetic and diamagnetic forms of force.'

These experiments present grave mechanical difficulties, and are quite sufficient to justify the conclusion drawn from them, namely, that the force which produces them is *neither* attractive *nor* repulsive. We will now endeavour to apply the idea of a force which is *both* attractive *and* repulsive, or in other words of a *polar* force, to the solution of the difficulty.

For the sake of disencumbering the mind of all considerations save those which belong to pure magne-crystallic action, we will suppose, as before, the bodies experimented with to be spherical.

Fig. 3.

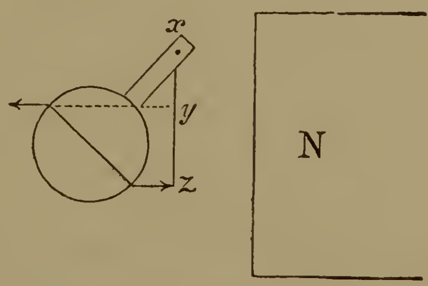


Let the dot at x, fig. 3, be the intersection of the vertical silken axis with Mr. Faraday's strip of card; and on the end of the strip, let the sphere of sulphate of iron be placed with its magne-crystallic axis ab at right angles to the length of the strip. This line, as I have already shown,* is that of most intense magnetisation through the crystal. The forces acting on the sphere in its present position are exactly similar to those acting upon the carbonate of iron in fig. 2. A residual 'couple' will apply itself at the extremities of ab, as indicated by the arrows, and would, if the sphere were free to turn round its centre of gravity, set the line ab parallel to the magnetic axis. But the sphere is here rigidly connected with a lever movable round its own axis of suspension, and it is easy to state the mechanical result that must follow from this arrangement. To obtain the 'moments' of the two forces acting upon a and b, we have to multiply each of them by its distance of its point of application from the axis x. Now in front of a flat

* Phil. Mag. S. 4, vol. ii. p. 178, and at p. 53 of this volume.

pole such as that made use of by Mr. Faraday in these experiments, the force diminishes very slowly as we recede from the pole. The consequence is that the attraction of a does not so far exceed the repulsion of b as to prevent the product of the latter into xz from exceeding that of the former into xy, and consequently the paramagnetic sphere must recede from the pole.*

Fig. 4.



In his next experiment, Mr. Faraday removed the pole S and allowed the pole N to act upon the crystal as in fig. 4. In this case it will be seen that the end nearest the pole, and therefore the most strongly attracted, is also at the greatest distance from the axis of rotation. Hence the sphere must approach the pole, as it does in the experiment.

When the strip of card is revolved 90°, we have the state of things shown in fig. 5; and when it is revolved 180°, we have the state of things shown in fig. 6. It is manifest, for the

Fig. 5.

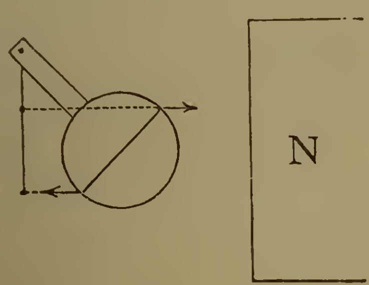
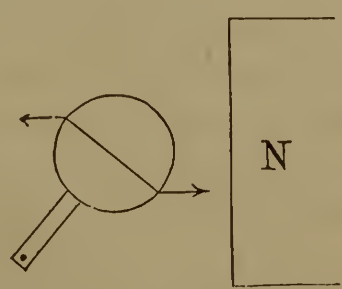


Fig. 6.

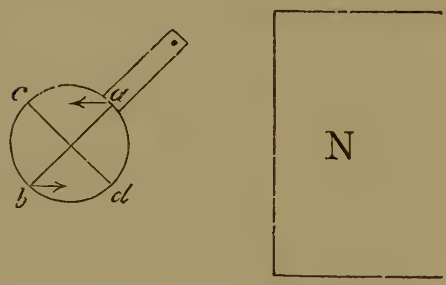


mechanical reasons already assigned, that the crystal, in both these cases, must recede from the pole.

* [Calling the attraction a, the force with which the sphere tends to turn *towards* the magnet is equal to $a \times xy$. Calling the repulsion r, the force with which the sphere tends to retreat from the magnet is $r \times xz$. If a be not much greater than r, the product $r \times xz$ will exceed $a \times xy$, and the sphere, *though magnetic*, will retreat as if repelled by the pole.]

Substituting for the sphere of sulphate of iron a sphere of bismuth with its magne-crystallic axis *cd*, fig. 7, perpendicular to the strip of card, the bismuth is found to approach the pole when the magnet is excited. The line *ab*, perpendicular to that called the magne-crystallic axis, has been shown by Mr. Faraday to be that of greatest diamagnetic intensity; the mass is therefore under the influence of forces precisely similar to those acting on the carbonate of lime in fig. 1. A residual couple, as denoted by the arrows, will act at the extremities of the line *ab*. The absolute repulsion of *a* in the field of force here assumed, does not differ much from the absolute attraction of *b*; but the latter force acts at the end of a much longer lever, and consequently the sphere is drawn towards the excited pole. I cannot help remarking here upon the severe faithfulness with which these results are recorded, and on the inestimable value of such records to scientific progress. The key to their solution being once found, the investigator may proceed confidently to the application of his principles, without fear of check or perplexity arising from the imperfection of his data.

Fig. 7.



In all these cases we have assumed that the magnetic force *diminishes slowly as we recede from the pole*, for this is essential to the production of the effects. The exact expression of the condition is, *that the advantage due to the proximity of the part of the mass nearest the pole, must be less than that arising from the greater leverage possessed by the force acting on the more distant parts*. When the shape of the poles is such that the diminution of the force with the increase of distance is too speedy for the above condition to be fulfilled, the phenomena no longer exhibit themselves. It is plain that the diminution of the force as we recede from a pointed pole must be more rapid, than when we recede from a magnetised surface, and hence it is that Mr. Faraday finds that 'conical poles are not good.' It is also essential that the length of the lever which supports the magne-

crystallic body shall bear a sensible ratio to the distance between the two points of application of the magnetic force. If the lever be long, *recession* will take place in cases where, with a shorter lever, *approach* would be observed.

It is well known that a piece of soft iron is attracted most strongly by the angles and corners of a magnet, and hence it is sometimes inferred that the magnetic force emanating from these edges and corners is more intense than that issuing from the central parts of the polar surfaces. Such experiments, however, when narrowly criticised, do not justify the inference drawn from them. They simply show that the *difference* between attraction and repulsion, on which the final attraction depends, is greater at the edges than elsewhere; but they do not enable us to infer the absolute strength of either the attraction or the repulsion, or in other words, of the force of magnetisation. The fact really is, *that while the attraction of the mass is nearly absent in the central portion of a magnetic field bounded by two flat poles, the magnetisation is really stronger there than between the edges.* This is proved by the following experiment:—

I suspended a cube of crystallised bismuth from a fibre of cocoon silk; when the magnet was excited, the cube set its planes of principal cleavage equatorial. When drawn aside from this position and liberated, it oscillated to and fro through it. Between the upper edges of the movable poles the number of oscillations performed in a minute was seventy-six; in the centre of the field the number performed was eighty-eight, and between the lower edges eighty. A cube of magnetic slate, similarly suspended, oscillated in the centre of the field forty-nine times, and between the edges only forty times, in fifteen seconds. In the former position there was no sensible tendency of the cube to move towards either pole; but in the latter position, though the magnetisation was considerably less intense, the cube was with difficulty prevented from moving up to one or the other of the edges. The reason of all this manifestly is, that while the forces in the centre of the field nearly neutralise each other as regards the *translation* of the mass, they are effective in producing its *oscillation*; while between the edges, though the absolute forces acting on the north and south poles of the excited substances are less intense, the *difference* of these forces, owing to the speedier diminution

of the force with the distance, is greater than in the centre of the field. It is therefore an error to infer, that, because the *attraction* of the mass is greater at the edges and corners than in the centre of the field, the *magnetising force* of the former must therefore be more intense than that of the latter.*

There is another interesting and delicate experiment of Mr. Faraday's to which I am anxious to apply the principle of diamagnetic polarity: the experiment was made with a view of proving that 'the magne-crystallic force is a force acting at a distance.' 'The crystal,' writes Mr. Faraday, 'is moved by the magnet at a distance, and the crystal can also move the magnet at a distance. To produce the latter result, I converted a steel bodkin, 3 inches long, into a magnet, and then suspended it vertically by a cocoon filament from a small horizontal rod, which again was suspended by its centre and another length of cocoon filament, from a fixed point of support. In this manner the bodkin was free to move on its own axis, and could also describe a circle about $1\frac{1}{2}$ inch in diameter; and the latter motion was not hindered by any tendency of the needle to point under the earth's influence, because it could take any position in the circle and yet remain parallel to itself.

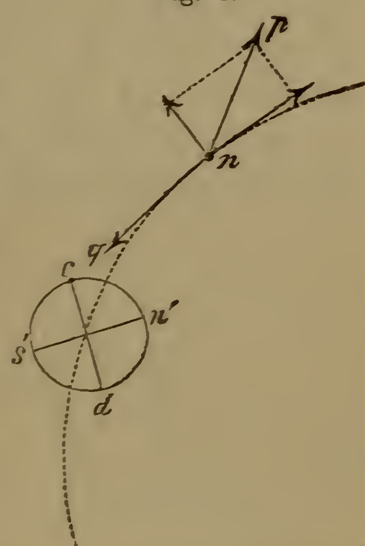
'When a crystal of bismuth was fixed on a support with the magne-crystallic axis in a horizontal direction, it could be placed near the lower pole of the magnet in any position; and being then left for two or three hours, or until by repeated examination the magnetic pole was found to be stationary, the place of the latter could be examined, and the degree and direction in which it was affected by the bismuth ascertained. . . . The effect produced was small; but the result was, that if the direction of the magne-crystallic axis made an angle of 10°, 20°, or 30° with the line from the magnetic pole to the middle of the bismuth crystal, then the pole followed it, tending to bring the two lines into parallelism; and this it did whichever end of the magne-crystallic axis was towards the pole, or whichever side it was inclined to. By moving the bismuth at successive times, the deviation of the magnetic pole could be carried up to 60°. The crystal, therefore, is able to react upon

* Some important consequences resulting from this experiment are intended for a future communication.

the magnet at a distance. But though it thus takes up the character of a force acting at a distance, still it is due to that power of the particles which makes them cohere in regular order, and gives the mass its crystalline aggregation; which we call at other times the attraction of aggregation, and so often speak of as acting at *insensible* distances.'

The disposition of this important experiment will be manifest from fig. 8, where *cd* is the magne-crystallic axis of a sphere of bismuth, or the line in which the diamagnetic induction is least intense; and *s′n′* the direction of the principal cleavage, or that of most intense diamagnetisation. Let *n* be the point of the bodkin, say its north pole, the crystal will be excited by the influence of this pole, and the resultant action will be the same as if it were exclusively 'diamagnetised' along the line *s′n′*. At the end nearest to the pole of the bodkin a repelled pole *n′* will be excited in the bismuth; at the most distant end an attracted pole *s′* will be excited. Let the repulsive force tending to separate *n* from *n′* be represented by the line *np*, and let the attraction exerted between *s′* and *n* be represented by the line *nq*; the arrangement is such that the force of *s′* acts more nearly in the direction of the tangent than that of *n′*; the latter may be decomposed into two, one acting along the circle and the other across it: the later component exerts a pressure against the axis of suspension; the former only is effective in causing the pole *n* to move; so that the whole, or nearly the whole, of the attraction has to compete with a comparatively small component of the repulsion. The former therefore preponderates, and the pole *n* approaches the crystal. It is manifest that as the angle which the line from *n* to the centre of the crystal makes with the magne-crystallic axis, increases, the component of repulsion which acts in the direction of a tangent to the curve, augments also; and that at a certain point this component must become preponderant. Beyond an angle of 30° it is to be presumed that

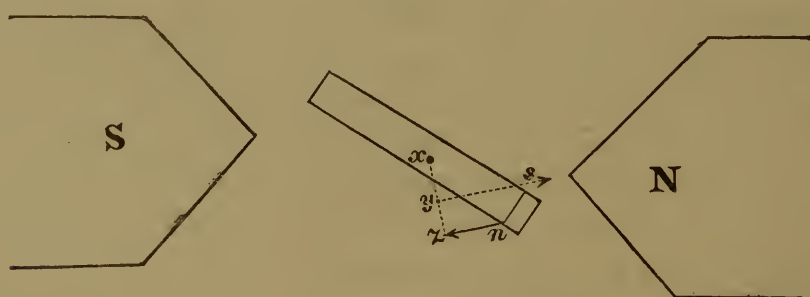
Fig. 8.



Mr. Faraday did not obtain the effect. Removing the crystal, and placing a small magnet in the position of the line $s' n'$, with its poles arranged as in the figure, the same phenomena would be produced.*

As finally illustrative of the sufficiency of the principle of polarity to explain the most complicated phenomena of magne-crystallic action, let us turn to the consideration of those curious effects of rotation first observed by M. Plücker, and illustrated by thirty-seven cases brought forward in the Bakerian Lecture for 1855. The effects, it will be remembered, consisted of the turning of elongated paramagnetic bodies suspended between *pointed poles* from the axial to the equatorial position, and of elongated diamagnetic bodies, from the equatorial to the axial position, when the distance between the suspended body and the poles was augmented. This is a subject of considerable difficulty to many, and I therefore claim the indulgence of those who have paid more than ordinary attention to it, if in this explanation I should appear to presume too far on the reader's want of acquaintance with the question. Let us then suppose an elongated crystal of tourmaline, staurolite, ferrocyanide of potassium, or beryl, *cr*, to be suspended between the conical poles N, S, fig. 9, of an electro-magnet; supposing the position between the poles to be the oblique one shown in the figure, let us inquire what are the forces acting upon the crystal in this position. In the case of all paramagnetic crystals which exhibit the phenomenon of rotation, it will be borne in mind that the line of most intense magnetisation is at right angles to the length of

Fig. 9.



* As there are no measurements given of the distances between the crystal and the pole, it is of course impossible to do more than indicate generally the theoretic solution of the experiment.

the crystal. Let *sn* be any transverse line near the end of the crystal; fixing our attention for the present on the action of the pole N, we find that a friendly pole is excited at *s* and a hostile pole at *n*: let us suppose *s* and *n* to be the points of application of the polar force, and, for the sake of simplicity, let us assume the distance from the point of the pole N to *s* to be half of the distance from N to *n*. We will further suppose the action of the pole to be that of a magnetic point, to which, in reality, it approximates; then, inasmuch as the quantities of north and south magnetism are equal, we have simply to apply the law of inverse squares to find the difference between the two forces. Calling that acting on *s* unity, that acting on *n* will be $\frac{1}{4}$. Opposed to this difference of the absolute forces is the difference of their moments of rotation; the

Fig. 10.

force acting on *n* is applied at a greater distance from the axis of rotation, but it is manifest that to counterbalance the advantage enjoyed by *s*, on account of its greater proximity, the distance *x z* would require to be four times that of *x y*. Taking the figure as the correct sketch-plan of the poles and crystal, it is plain that this condition is not fulfilled, and that hence the end of the crystal will be drawn towards the pole N. What we have said of the pole N is equally applicable to the pole S, so that such a crystal suspended between two such poles, in the manner here indicated, will set its length along the line which unites them.

While the crystal retains the position which it occupied in fig. 9, let the poles be removed further apart, say to ten times their former distance. The ratio of the two forces acting on the two points of application *s* and *n* will be now as the square of 11 to the square of 10, or as 6 : 5 nearly. Taking fig. 10, as in the former case, to be the exact sketch of the crystal, it is

manifest that the ratio of $x\ z$ to $x\ y$ is greater than that of 6 to 5,* the advantage, on account of greater leverage, possessed by the force acting on n is therefore greater than that which greater proximity gives to s, and the consequence is that the crystal will recede from the pole, and its position of rest between two poles placed at this distance apart will be at right angles to the line which joins them. It is needless for me to go over the reasoning in the case of a diamagnetic body whose line of strongest diamagnetisation is perpendicular to its length. Reversing the direction of the arrows in the last two figures, we should have the graphic representation of the forces acting upon such a body; and a precisely analogous mode of reasoning would lead us to the conclusion, that when the polar points are near the crystal, the latter will be driven towards the equatorial position, while where they are distant, the crystal will be drawn into the axial position. In this way the law of action laid down empirically in the Bakerian Lecture for 1855 is deduced *à priori* from the polar character of both the magnetic and diamagnetic forces. The most complicated effects of magne-crystallic action are thus reduced to mechanical problems of extreme simplicity; and, inasmuch as these actions are perfectly inexplicable except on the assumption of diamagnetic polarity, they add their evidence in favour of this polarity to that already furnished in such abundance.

Perhaps as remarkable an illustration as could be chosen of the apparently perplexing character of certain magnetic phenomena, but of their real simplicity when the exact nature of the force producing them is understood, is furnished by the following experiment. I took a quantity of pure bismuth powder and squeezed it between two clean copper plates until the powder became a compact mass. A fragment of the mass suspended before the pointed pole of a magnet was forcibly repelled; and when suspended in the magnetic field with the direction of pressure horizontal, in accordance with results already sufficiently well known, it set its line of pressure equatorial.

A second quantity of the bismuth powder was taken, and with it was mixed a quantity of powdered carbonate of iron, amounting to $\frac{3}{10}$ths per cent. of the whole; the mass was still strongly

* At a distance, moreover, the whole mass of the pole, not its point alone, comes into play.

diamagnetic, but the line of compression, instead of setting equatorial as in the former instance, set decidedly axial.

A quantity of the mixed powder was next taken, in which the magnetic constituents amounted to 1 per cent. The mass was still diamagnetic, but the line of compression set axial; it did so when the influence of exterior form was quite neutralised, so that the effect must be referred solely to the compression of the mass. With 2 per cent. of carbonate of iron powder the mass was magnetic, and set with increased energy its line of compression axial; with 4 per cent. of carbonate of iron the same effect was produced in a still more exalted degree.

Now, why should the addition of a quantity of carbonate of iron powder, which is altogether insufficient to convert the mass from a diamagnetic to a paramagnetic one, be able to overturn the tendency of the diamagnetic body to set its line of compression equatorial? The question is puzzling at first sight, but the difficulty vanishes on reflection. The repulsion of the mass of bismuth, suspended before a pointed pole, depends upon the general capacity of the mass for diamagnetic induction, while its position as a magne-crystal between the flat poles depends on the difference between its capacities in two different directions. The diamagnetic capacity of the mass may be very great while its capacity in different directions may be nearly alike, or quite so: the former, in the case before us, came into play before the pointed pole; but between the flat poles, where the *directive*, and not the *translative* energy is great, the carbonate of iron powder, whose directive power, when compressed, far exceeds that of bismuth, determined the position of the body. In this simple way a number of perplexing results obtained with bodies formed of a mixture of paramagnetic and diamagnetic constituents, is capable of satisfactory explanation.

Finally, inasmuch as the set of the mass in the magnetic field depends upon the difference of its excitement in different directions, it will follow that any circumstance which affects all directions of a magne-crystallic mass in the same degree will not disturb the differential action upon which its deportment depends. This seems to me to be the explanation of the results recently obtained by Mr. Faraday with such remarkable uniformity, namely, that, no matter what the medium may be in which the magne-crystallic body is immersed, whether air or

liquid, paramagnetic or diamagnetic, it requires, in all cases, the same amount of force to turn it from the position which it takes up in virtue of its structure.*

I have thus dwelt upon instances of magne-crystallic action which have revealed themselves in actual practice, as affording the best examples for the application of the knowledge which the demonstration of the polarity of the diamagnetic force places in our possession; and I believe it has been shown that these phenomena, which were in the highest degree paradoxical when first announced, are deducible with as much ease and certainty from the action of polar forces, as the precession of the equinoxes is from the force of gravitation. *The whole domain of magne-crystallic action is thus transferred from a region of mechanical enigmas to one in which our knowledge is as clear and sure as it is regarding the most elementary phenomena of magnetic action.*

Royal Institution:
December, 1855.

* I need hardly draw attention to the suggestive beauty of this experiment.—J. T., 1870.

LETTERS, ESSAYS, AND REVIEWS

RELATING TO

MAGNETISM AND ELECTRICITY.

The following pages embrace letters on diamagnetism, some of which are of permanent interest; a number of essays, to be regarded in part as lecture-room summaries, useful, I trust, to the practical scientific teacher; two reviews relating to electricity, and extracted from a series which under the title 'Reports on the Progress of the Physical Sciences,' were written in those early days for the 'Philosophical Magazine.' The volume ends with a memoir on Electro-magnetic Attractions, intended to prepare me for the investigation of diamagnetic phenomena, but which of itself constituted a healthful discipline at the time.

I.—FARADAY ON MAGNETIC POLARITY.

[In the 'Philosophical Magazine' for February 1855, Faraday published an exceedingly interesting paper 'On Some Points of Magnetic Philosophy,' in which he discusses the question of Diamagnetic Polarity, and also describes some beautiful experiments relating to the influence of various media surrounding magnetic and diamagnetic bodies. He then applies his experiments to support a theory of diamagnetic polarity similar to that advocated by Edmond Becquerel, and referred to at page 47 of this book. The portion of Faraday's paper necessary to explain what follows is here given.—J. T., 1870.]

The meaning of this phrase is rapidly becoming more and more uncertain. In the ordinary view, polarity does not necessarily touch much upon the idea of lines of physical force; yet in the one natural truth it must either be essential to, and identified with it, or else absolutely incompatible with, and opposed to it. Coulomb's view makes polarity to depend upon the resultant in direction of the action of two separated and distant portions of two magnetic fluids upon other like separated portions, which are either originally separate, as in a magnet, or are induced to separate, as in soft iron, by the action of the dominant magnet;—it is essential to this hypothesis that the polarity force of one name should repel polarity force of the same name and attract that of the other name. Ampère's view of polarity is, that there are no magnetic fluids, but that closed currents of electricity can exist round particles of matter (or round masses), and that the known experimental difference on the opposite sides of these currents, shown by attraction and repulsion of other currents, constitutes polarity. Ampère's view is modified (chiefly by addition) in various ways

by Weber, De la Rive, Matteucci, and others.* My view of polarity is founded upon the character in direction of the force itself, whatever the cause of that force may be, and asserts that when an electro-conducting body moving in a constant direction near or between bodies acting magnetically on themselves or each other has a current in a constant direction produced in it, the magnetic polarity is the same; if the motion or the current be reversed, the contrary polarity is indicated. The indication is true either for the exterior or the interior of magnetic bodies whenever the electric current is produced, and depends upon the unknown but essential dual or antithetical nature of the force which we call magnetism (3154).

The numerous meanings of the term polarity, and various interpretations of polarity indications at present current, show the increasing uncertainty of the idea and the word itself. Some consider that the mere set or attraction, or even repulsion, shown by a body when subject to a dominant magnet is sufficient to mark polarity, and I think it is as good a test as any more refined arrangement (2693) when the old notion of polarity only is under consideration. Others require that two bodies under the power of a dominant magnet should by their actions show a mutual relation to each other before they can be considered as polar. Tyndall, without meaning to include any idea of the nature of the magnetic force, takes his type from soft iron, and considers that any body presenting the like or the antithetical phenomena which such iron would present under magnetic action, is in a like or antithetical state of polarity.† Thomson does not view two bodies which present these antithetical positions or phenomena as being necessarily the reverse of each other in what may be called their polar states,‡ but, I think, looks more to differential action, and in that approaches towards the views held generally by E. Becquerel and myself. Matteucci considers that the whole mass of the polar body ought to be in dependence by its particles as a mass of iron is, and that a solution of iron and certain salts of

* I find the following marginal note written by myself in 1855, and referring to this place:—'These hypotheses are so many efforts to assign the *cause* of polarity. Polarity itself is not a hypothesis, but a fact.' Indeed until we know what magnetism itself is we shall not be able to assign the physical cause of its polarity—J. T., 1870.

† *Athenæum*, No. 1406, p. 1203.

‡ Ibid. column 3 at bottom.

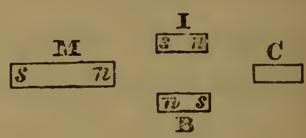
iron have not poles, properly speaking, but that at the nearest points to the dominant pole there is the contrary magnetism to that of the pole, surrounded by the same magnetism as of the pole in the further part, the two ends of a bar of such matter between two dominant poles having no relation to each other.* Becquerel considers that polarity may in certain cases occur transverse to the length, and so produce results which others explain by reverse polarity. The views of very many parties always include the idea of the source of the polar action, whether that be supposed to depend on the accumulation of magnetic fluids at the chief poles of the dominant magnet, or the action of electric currents in a determinate position around its molecules; and such views are adhered to even when the polarity induced is of the reverse kind, as in bismuth, &c., to that of the inducing magnet. Others, like Weber, add to Ampère's hypothesis an idea of electricity, loose as regards the particles, though inseparably associated with the mass of the body under induction. Some, I think, make the polarity not altogether dependent upon the dominant magnet, but upon the neighbouring or surrounding substances; and I propose, if the physical lines of force should hereafter be justified, to make that which is commonly called polarity, in distinction from the true polarity (3307), dependent upon the better or worse magneto-conduction power of the substances presenting the usual polar phenomena (2818).

The views of polar action and of magnetism itself, as formerly entertained, have been powerfully agitated by the discovery of diamagnetism. I was soon driven from my first supposition, that the N pole of a magnet induced like or N polarity in the near part of a piece of bismuth or phosphorus; but as that view has been sustained by very eminent men, who tie up with it the existence of magnetic fluids or closed electric currents as the source of magnetic power, it claims continued examination, for it will most likely be a touchstone and developer of real scientific truth, whichever way the arguments may prevail. To me the idea appears to involve, if not magnetic impossibilities, at least great contradiction and much confusion, some of which I proceed to state, but only with the desire of elucidating the general subject.

* *Cours spécial sur l'Induction, &c.*, p. 201.

If an ordinary magnet M, fig. 1, acting upon a piece of iron or other paramagnetic matter I, renders it polar by throwing its near end into the contrary or S state in the manner usually understood, and, acting upon a like piece of diamagnetic matter as bismuth B, renders it also polar, but with the near end in the same state; then B and I are for the time two magnets, and must act back upon the magnet M; or if they could be made able to retain their states after M is removed (and that is the case with I), would act as magnets upon a third piece of magnetic matter as C. When M acts upon I, it exerts its influence, according to the received theories, upon all the particles of the latter, bringing them into like polar position with itself, and these, consistently with the simple assumption, act also upon each other as particle magnets, and exalt the polarity of the whole mass in its two extremities. In like manner M should act upon B, polarising the mass and all its particles; for the particles of the diamagnetic body B, even to the smallest, must be operated upon; and we know experimentally, that a tube filled with powdered bismuth acts as a bar of the metal does. But then what is the mutual action of these bismuth particles on each other? for though all may be supposed to have a reverse polarity to that of M, they cannot in that case be reverse in respect of each other. All must have like polarity, and the N of one particle must be opposed to the S of the next particle in the polarity direction. That these particles act on each other, must be true, and Tyndall's results on the effect of compression have proved that by the right means, namely, experiment. If they were supposed to have no such action on each other, it would be in contradiction to the essential nature of magnetic action, and there would remain no reason to think that the magnet itself could act on the particles, or the particles react on it. If they acted on each other as the magnet is supposed to act on them, i.e. to induce contrary poles, then the power of the magnet would be nullified, and the more effectually the nearer the particles were together; whereas Tyndall has shown that the bismuth magnetic condition is exalted by such vicinity of the particles, and hence we have a further right to conclude that they do act on, or influence each

Fig. 1.



other, to the exaltation of the state of the mass. But if the N-ness of one particle corresponds to, and aids in sustaining and exalting, the S-ness of the next particle, the whole mass must have the same kind of force, so that, as a magnet, its polarity must have the same kind of polarity as that of the particles themselves. For whether a particle of bismuth be considered as acting upon a neighbouring particle or upon a distant particle of bismuth, or whether a mass of particles be considered as acting on the distant particle, the action in both cases must be precisely of the same kind.

But why should a polarised particle of bismuth acting upon another particle of bismuth produce in it like polarity, and with a particle of iron produce a contrary polarity? or why should masses of bismuth and iron, when they act as magnets (3310), produce such different effects? If such were the case, then the N pole of a paramagnetic body would induce an S pole on the near end of an iron rod, whilst the N pole of a diamagnetic body would produce a pole contrary to the former, i.e. an N pole at the same end of the iron rod in the same position and place. This would be to assume two kinds of magnetism, i.e. two north fluids (or electric currents) and two south; and the northness of bismuth would differ from the northness of iron as much as pole from pole. Still more, the northness of bismuth and the southness of iron would be found to have exactly like qualities in all points, and to differ in nothing but name; and the southness of bismuth and northness of iron would also prove to be absolutely alike. What is this, in fact, but to say they *are* the same? and why should we not accept the confirmation and unfailing proof that it is so, which is given to us experimentally by the moving wire? (3307, 3356.)

If we employ a magnet as the originally inducing body (3310), and entertain the idea of magnetic fluids accumulated at the poles, which act by their power of attracting each other, but repelling their like, then the inconsistency of supposing that the north fluid of a given pole can attract the north fluid of one body and the south fluid of another, or that the north and south fluids of the dominant magnet can attract one and the same fluid in bismuth and in iron, &c., is very manifest. Or if we act by a solenoid or a helix of copper wire carrying an electric current instead of a magnet, and find that analogous

effects are produced, are we to admit at once that the electric currents in it, acting upon the assumed electric circuits round the particles of matter, sometimes attract them on the one side and sometimes on the other? or if such bodies as bismuth and platinum are put into such a helix, are we to allow that currents in opposite directions are induced in them by one and the same inducing condition? and that, too, when all the other phenomena, and there are many, point to a uniformity of action as to direction with a variation only in power.

Media.

Let us now consider for a time the action of different *media*, and the evidence they give in respect of polarity. If a weak solution of protosulphate of iron,* *m*, be put into a selected thin glass tube about an inch long, and one-third or one-fourth of an inch in diameter, and sealed up hermetically (2279), and be then suspended horizontally between the magnetic poles in the air, it will point axially, and behave in other respects as iron; if, instead of air between the poles, a solution of the same kind as *m*, but a little stronger, *n*, be substituted, the solution in the tube will point equatorially, or as bismuth. A like solution somewhat weaker than *m*, to be called *l*, enclosed in a similar tube, will behave like bismuth in air but like iron in water. Now these are precisely the actions which have been attributed to polarity, and by which the assumed reverse polarities of paramagnetic and diamagnetic bodies have been considered as established; but when examined, how will ideas of polarity apply to these cases, or they to it? The solution *l* points and acts like bismuth in air and like iron in water; are we then to conclude that it has reverse polarity in these cases? and if so, what are the reasons and causes for such a singular contrast in that which must be considered as dependent upon its internal or molecular state?

In the first place, no want of magnetic continuity of parts can have anything to do with the inversion of the phenomena; for it has been shown sufficiently by former experiments,†

* Let *l* contain 4 grains, *m* 8 grains, *n* 16 grains, and *o* 32 grains of crystallised protosulphate of iron in each cubic inch of water.

† Phil. Mag. 1846, vol. xxix. p. 254.

that such solutions are as magnetically continuous in character as iron itself.

In the next place, I think it is impossible to say that the medium interposed between the magnet and the suspended cylinder of fluid can cut off, or in any way affect the direct force of the former on the latter, so as to change the direction of its internal polarity. Let the tube be filled with the solution *m*, then if it be surrounded by the solution *l*, it will point as iron; if the stronger solution *n* surround it, it will point as bismuth; and with sufficient care a succession of these fluids may be arranged as indicated in figs. 2, 3, where the outlines between the poles represent the forms of thin glass troughs, and the letters the solutions in them. In fig. 2 we see that the action on *m* is the same as that on *m′*, and the pointing of the two portions is the same, i.e. equatorial; neither has the action on *m* been altered by the power of the poles having to traverse *n*, *m′* and *n′*; and in fig. 3 we see, that, under like circumstances of the power, *m′* points as bismuth and *m* as iron, though they are the same solution with each other and with the former *m m′* solutions. No cutting off of power by the media could cause these changes; repetitions of position in the first case, and inversions in the second. All that could be expected from any such interceptions would be perhaps diminutions of action, but not inversions of polarity; and every consideration indicates that all the portions of these solutions in the field at once have *like polarity*, i. e. like direction of force through them, and like internal condition; each solution in its complex arrangement being affected exactly in the same way and degree as if it filled the whole of the magnetic field, although in these particular arrangements it sometimes points like iron, and at other times like bismuth (2362, 2414).

Fig. 2.

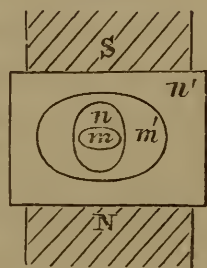
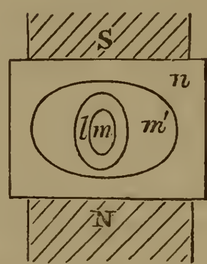


Fig. 3.



These motions and pointings of the same or of different solutions, contain every action and indication which is supposed to distinguish the contrary polarities of paramagnetic and diamagnetic bodies from each other, and the solutions *l* and *m* in air repeat exactly the phenomena presented in air by phos-

phorus and platinum, which are respectively diamagnetic and paramagnetic substances. But we know that these actions are due to the differential result of the masses of the moving or setting solution and of that (or the air) surrounding it. No structural or internal polarity, having opposite directions, is necessary to account for them (2361, 2757). If, therefore, it is still said that the solution *m* has one polarity in *l* and the reverse polarity in *n*, that would be to make the polarity depend upon the *mass* of *m* independently of its particles; for it can hardly be supposed that the particles of *m* are more affected by the influence upon them of the surrounding medium (itself under like inductive action only, and almost insensible as a magnet) than they are by the dominant magnet.* It would be also to make the polarity of *m* as much, or more, dependent upon the surrounding medium than upon the magnet itself;—and it would be, to make the masses of *m* and *l* and even their *form* the determining cause of the polarity; which would remove polarity altogether from dependence upon internal molecular condition, and, I think, destroy the last remains of the usual idea. For my own part, I cannot conceive that when a little sphere of *m* in the solution *l* is attracted upon the approach of a given magnetic pole, and repelled under the action of the same pole when it is in the solution *n*, its particles are in the two cases polar in two opposite directions; or that if for a north magnetic pole it is the *near* side of the particles of *m* when in *l* that assume the south state, it is the *further* side which acquires the same state when the solution *l* is changed for *n*. Nor can I think that when the particles of *m* have the same polar state in both solutions, the whole, as a mass, can have the opposite states.

These differential results run on in one uninterrupted course from the extreme of paramagnetic bodies to the extreme of diamagnetic bodies; and there is no substance within the series which, in association with those on each side of it, may not be made to present in itself the appearances and action

* If the polarity of the inner mass of solution is dependent upon that of the outer, and cannot be affected but through it, then why is not air and space admitted as being in effective magnetic relation to the bodies surrounded by them? How else could a distant body be acted upon by a magnet, if the inner solution of sulphate of iron is so acted on? Are we to assume one mode of action by contiguous masses of particles in one case, and another through distance in another case?

which are considered as indicating the opposite polarities of iron and bismuth. How then is their case, in the one or the other condition, to be distinguished from the assumed polarity conditions of bismuth or of iron?—only, I think, by assuming other points which beg the whole question. In the first place, it must be, or is assumed, that no magnetic force exists in the space around a magnet when it is in a vacuum, it being denied that the power either crosses or reaches a locality in that space until some material substance, as the bismuth or iron, is there. It is assumed that the space is in a state of magnetic darkness (3305), an assumption so large, considering the knowledge we have of natural powers, and especially of dual forces, that there is none larger in any part of magnetic or electric science, and is the very point which of all others should be held in doubt and pursued by experimental investigation. It is as if one should say, there is no light or form of light in the space between the sun and the earth, because that space is invisible to the eye. Newton himself durst not make a like assumption even in the case of gravitation (3305), but most carefully guards himself and warns others against it, and Euler * seems to follow him in this matter. Such an assumption, however, enables the parties who make it to dismiss the consideration of differential effects when bodies are placed in a vacuum, and to divide the bodies into the well-known double series of paramagnetic and diamagnetic substances. But in the second place, even then, those who assume the reverse polarity of diamagnetic bodies, must assume also that the state set up in them by conduction is less favourable to either the exercise or the transmission of the magnetic force than the original unpolarised state of the bismuth; an assumption which is, I think, contrary to the natural action and final stable condition into which the physical forces tend to bring all bodies subject to them. That a magnet acting on a piece of iron should so determine and dispose of the forces as to make the magnet and iron mutually accordant in their action, I can conceive; but that it should throw the bismuth into a state which would make it repel the magnet, whereas if unaffected it should be so far favourable as to be at least indifferent, is what I cannot imagine to myself. In the third place, those who rest

* Letters, &c. translated. Letter LXVIII., or pp. 260–262.

their ideas on *magnetic fluids*, must assume that in all diamagnetic cases, and in them only, the fundamental idea of their mutual action must not only be set aside but inverted, so that the hypothesis would be at war with itself; and those who assume that *electric currents* are the cause of magnetic effects, would have to give up the law of their inducing action (as far as we know it) in all cases of diamagnetism, at the very same moment when, if they approached the diamagnetic bismuth in the form of a spiral to the pole, they would have a current produced in it *according* to that law.

Time.

I will venture another thought or two regarding the condition into which diamagnetic bodies are brought by the act of magnetic induction, in connection with the point of *time*. It appears, as far as I remember, that all natural forces tend to produce a state of rest, except in cases where vital or organic powers are concerned; and that as in *life* the actions are for ever progressive, and have respect to a future rather than a present state (Paget), so all *inorganic* exertions of force tend to bring about a stable and permanent condition, having as the result a state of rest, i.e. a static condition of the powers.

Applying this consideration to the case of bismuth in the magnetic field, it seems to me more like the truth of nature that the state assumed by the bismuth should be one more favourable to the final and static exercise of the power of the dominant magnet upon it, than that state belonging to the bismuth before it had suffered or undergone the induction; exactly as in soft iron we know that before it has acquired the state which a dominant magnet can induce upon it, it is not so favourable to the final static condition of the powers as it is afterwards. Now it is very manifest by numerous forms of experiment, that *time* enters as an element into ordinary magnetic and magneto-electric actions, and there is every reason to expect into diamagnetic actions also; and it is also well known that we can take advantage of this time, and test the state of a piece of iron in the magnetic field before it has attained its finally induced state, and afterwards;—as, for instance, by placing it with a helix round it in the magnetic field and quickly connecting the helix *afterwards* with a galvanometer,

when a current of electricity in such direction as to prove the truth of the statement will be obtained. In other forms of experiment, and with large pieces of iron, the time which can be so separated or snatched up during the act of progressive induction will amount to a minute or more. Supposing this could be done in any sensible degree with diamagnetic bodies, then the following considerations present themselves. A globe or bar of bismuth in the magnetic field may have its states, before and after induction, considered as separated by a moment of time; if the induction raises up a state of polarity the reverse of that of the magnet, then the bismuth ought to be more favourable to the determination of magnetic force upon it *before* the induction than *after*; whereas if, according to my view, the polarity is not reversed, but is the same as that of the magnet, the metal ought to be more favourable to the determination of magnetic force upon or through it *after* induction than *before*. Believing this to be an experiment which would settle the question of reverse polarity, and perhaps the existence or non-existence of physical lines of magnetic force, I have made many attempts in various ways, and especially by alternating motions of cylinders and balls of bismuth between soft iron magnetic poles furnished with helices, to obtain some results due to the *time* of induction, but have been as yet unable to succeed. I cannot doubt that time is concerned; but it seems to be so brief in period as to be inappreciable by the means I have employed.

Professor Thomson has put this matter of time and polarity in another form. If a globe of bismuth be placed without friction in the middle of the magnetic field, it will not point or move because of its shape; but if it have reverse polarity, it will be in a state of unstable equilibrium; and if *time* be an element, then the ball, being once moved on its axis ever so little, would then have its polarity inclined to the magnetic axis, and would go on revolving for ever, producing a perpetual motion. I do not see how this consequence can be avoided, and therefore cannot admit the principles on which it rests. The idea of a perpetual motion produced by static forces is philosophically illogical and impossible, and so I think is the polarly opposed or adverse static condition to which I have already referred.

It is not necessary here that I should refer to the manner in which my view of the lines of magnetic force meet these cases, for it has been done in former papers (2797, &c.); but I will call the attention of those who like to pursue the subject, to a true case of reverse polarity in the magnetic field (Experimental Researches, 3238, fig. 15), and there they will easily see and comprehend the beginning of the rotation of Professor Thomson's bismuth globe, and its continuance, if, as supposed, the polar state represented in the figure could be continually renewed.

When the north pole of a magnet repels a piece of bismuth in a vacuum, or makes a bar of it set equatorially, and is found to produce like actions with many paramagnetic bodies when surrounded by media a little more paramagnetic than themselves, and with as many diamagnetic bodies when surrounded by media a little less diamagnetic, it would seem more cautious in the first instance to inquire how these latter motions take place, and how it is that parts, which with the paramagnetics have certainly been brought into a south condition by the north end of the pole, recede from it; and to apply these results in the first instance to those obtained with bismuth in a vacuum, before we assume a total change in principle, and yet an exceptional change as to substances, in the general law of magnetic polarity, without any [other] cause assigned than, or any supporting facts beyond, the effect in question.

II.—ON THE EXISTENCE OF A MAGNETIC MEDIUM IN SPACE.

[‘These motions and pointings,’ says Faraday, p. 207, ‘contain every action and indication which is supposed to distinguish the contrary polarities of paramagnetic and diamagnetic bodies.’ In the following letter I ventured to draw his attention to certain phenomena which the motions and pointings referred to did not seem to cover. See ‘Philosophical Magazine,’ vol. ix. p. 205.—J. T., 1870.]

My dear Mr. Faraday,—Few, I imagine, who read your memoir in the last number of the ‘Philosophical Magazine,’ will escape the necessity of reconsidering their views of magnetic action. We are so accustomed to regard the phenomena of this portion of science through the imagery with which hypothesis has invested them, that it is extremely difficult to detach symbols from facts, and to view the latter in their purity. This duty, however, is now forced upon us; for the more we reflect upon the results of recent scientific research, the more deeply must we be convinced of the impossibility of reconciling these results with our present theories.* In the downfall of hypotheses thus pending, the great question of a universal magnetic medium has presented itself to your mind. Your researches incline you to believe in the existence of such a medium, and lead you, at the same time, to infer the perfect identity of magnetism and diamagnetism.

In support and illustration of your views, you appeal to the following beautiful experiments:—Three solutions of protosulphate of iron are taken; the first, *l*, contains 4 grains; the second, *m*, 8 grains; and the third, *n*, 16 grains of the salt to a cubic inch of water. Enclosed in hollow globules of glass, all these solutions, when suspended in the air before the pole of a magnet, are attracted by the pole. You then place a quantity

* Some of the reasons which induce the writer to hold this opinion are given in the Bakerian Lecture of the Royal Society for the present year (1855).

of the medium solution, *m*, in a proper vessel, immerse in it the globule containing the strong solution, *n*, and find that the latter is still attracted; but that when the globule containing the solution *l* is immersed, the latter is *repelled* by the magnetic pole. Substituting elongated tubes for spheres, you find that when a tube containing a solution of a certain strength is suspended in a weaker solution, between the two poles of a magnet, the tube sets from pole to pole; but that when the solution *without* the tube, is stronger than that *within* it, the tube recedes from the pole and sets equatorial.

Here then, you state, are the phenomena of diamagnetism. It is maintained by some, that, to account for these phenomena, it is necessary to assume, in the case of diamagnetic bodies, the existence of a polarity the reverse of that of iron. But nobody will affirm that the mere fact of its being suspended in a stronger solution reverses the polarity of a magnetic liquid: —to account for the repulsion of the weak solution, when submersed in a stronger one, no such hypothesis is needed; why then should it be thought necessary in the case of so-called diamagnetic bodies? It is only by denying that space presents a medium which bears the same relation to diamagnetic bodies that the stronger magnetic solution bears to the weaker one, that the hypothesis of a distinct diamagnetic polarity is at all rendered necessary.

The effects upon which the foregoing striking argument is based are differential ones, and are embraced, as already observed by M. E. Becquerel, by the so-called principle of Archimedes. This principle, in reference to the case before us, affirms that the body immersed in the liquid is attracted by a force equal to the difference of the attractions exerted upon the liquid and the body immersed in it. Hence, if the attraction of the liquid be less than that of the immersed body, the latter will approach the pole; if the former attraction be the greater, the immersed body recedes from the pole, and is apparently repelled. The action is the same as that of gravity upon a body immersed in water: if the body be more forcibly attracted bulk for bulk, than the water, it sinks; if less forcibly attracted, it rises; the mechanical effect being the same as if it were repelled by the earth.

The question then is, are all magnetic phenomena the result of a differential action of this kind? Does space present a

medium less strongly attracted than soft iron, and more strongly attracted than bismuth, thus permitting of the approach of the former, but causing the latter to recede from the pole of a magnet? If such a medium exists, then diamagnetism, as you incline to believe, merges into ordinary magnetism, and 'the polarity of the magnetic force,' in iron and in bismuth, is one and the same.

Pondering upon this subject a few evenings ago, and almost despairing of seeing it ever brought to an experimental test, a thought occurred to me which, when it first presented itself, seemed to illuminate the matter. Such illuminations vanish in nine cases out of ten before the test of subsequent criticism; but the thought referred to, having thus far withstood the criticism brought to bear upon it, I am emboldened to submit it to you for consideration.

I shall best explain myself by assuming that a medium of the nature described exists in space, and pursuing this assumption to its necessary consequences.

Let a cube, formed from the impalpable dust of carbonate of iron,* which has been compressed forcibly in one direction, be placed upon the end of a torsion beam, and first let the line in which the pressure has been exerted be in the direction of the beam. Let a magnet, with its axis at right angles to the beam, and hence also at right angles to the line of pressure, be brought to bear upon the cube. The cube will be attracted, and the amount of this attraction, at any assigned distance, may be accurately measured by the torsion of the wire from which the beam depends. Let this attraction, expressed in degrees of torsion, be called a. Let the cube now be turned round 90°, so that the line of pressure shall coincide with the direction of the axis of the magnet, and let the attraction $\acute{a}$ in this new position be determined as in the former instance. On comparison it will be found that $\acute{a}$ exceeds a; or, in other words, that the attraction of the cube is strongest when the force acts parallel to the line of compression.†

Instead of carbonate of iron we might choose other substances of a much feebler magnetic capacity, with precisely the same

* For an ample supply of this most useful mineral, I am indebted to the kindness of J. Kenyon Blackwell, Esq., F.G.S.

† Phil. Mag. Sept. 1851; Pogg. *Ann.* 1851. The third memoir of this volume.

result. Let us now conceive the magnetic capacity of the compressed cube to diminish gradually, and thus to approach the capacity of the medium in which, according to our assumption, the carbonate of iron is supposed to be immersed. If it were a perfectly homogeneous cube, and attracted with the same force in all directions, we should at length arrive at a point, when the *magnetic weight* of the cube, if I may use the term, would be equal to that of the medium, and we should then have a substance which, as regards magnetism, would be in a condition similar to that of a body withdrawn from the action of gravity in Plateau's experiments. Such a body would be neither attracted nor repelled by the magnet. In the compressed cube, however, the magnetic weight varies with the direction of the force; supposing the magnetic weight, when the force acts along the line of compression, to be equal to that of the medium, then if the force acted across the line of compression, the magnetic weight of the cube would be less than that of the medium. Acted upon in the former direction, the cube would be a neutral body; acted upon in the latter direction, it would be a diamagnetic body. If the magnetic capacity of the cube diminish still further it will, according to your hypothesis, become wholly diamagnetic. Now it is evident, supposing the true magnetic excitement to continue, that the cube, when acted on by the magnet in the direction of compresssion, *will approach nearer to the magnetic weight of the medium* in which we suppose it immersed, than when the action is across the said line; and, hence, the repulsion of the cube, when the force acts along the line of compression, must be *less* than when the force acts across it.

Reasoning thus from the assumption of a magnetic medium in space, we arrive at a conclusion which can be brought to the test of experiment. So far as I can see at present, the assumption is negatived by this test; for in diamagnetic bodies the repulsion along the line in which the pressure is exerted is proved by experiment to be a *maximum*.* An ordinary magnetic excitement could not, it appears to me, be accompanied by this effect.

The subject finds further, and perhaps clearer elucidation in

* Phil. Mag. Sept. 1851. Pogg. *Ann.* 1851.

the case of isomorphous crystals. It is not, I think, questioned at present, that the deportment of crystals in the magnetic field depends upon their molecular structure; nor will it, I imagine, be doubted, that the molecular structure of a complete crystal of carbonate of iron is the same as that of an isomorphous crystal of carbonate of lime. In the architecture of the latter crystal, calcium simply takes the place which iron occupies in the former. Now a crystal of carbonate of iron is attracted most forcibly when the attracting force acts parallel to the crystallographic axis.* Let such a crystal be supposed to diminish gradually in magnetic capacity, until finally it attains a magnetic weight, *in a direction parallel to its axis*, equal to that of the medium in which we assume it to be immersed. Such a crystal would be indifferent, if the force acted parallel to its axis, but would be repelled, if the force acted in any other direction. If the magnetic weight of the crystal diminish a little further, it will be repelled in all directions, or, in other words, will become diamagnetic; but it will then follow, that the repulsion in the direction of the axis, if the nature of the excitement remain unchanged, will be less than in any other direction. In other words, a diamagnetic crystal of the form of carbonate of iron will, supposing magnetism and diamagnetism to be the same, be repelled with a *minimum* force when the repulsion acts parallel to the axis. Here, as before, we arrive at a conclusion which is controverted by experiment; for the repulsion of a crystal of carbonate of lime is a *maximum* when the repelling force acts along the axis of the crystal. Hence I would infer that the excitement of carbonate of iron cannot be the same as that of carbonate of lime.

Such are the reflections which presented themselves to my mind on the evening to which I have referred. I now submit them to you as a fraction of that thought which your last memoir upon this great question will assuredly awaken.

Believe me,

Dear Mr. Faraday,

Yours very faithfully,

JOHN TYNDALL.

ROYAL INSTITUTION:
February, 1855.

* Phil. Mag. Sept. 1851. Pogg. *Ann.* 1851.

III.—MAGNETIC REMARKS BY PROFESSOR FARADAY.

[To the foregoing letter Faraday returned the following reply. Phil. Mag. vol. ix. p. 253.]

My dear Tyndall,—In relation to your letter of last month,* I write, not for the purpose of giving what might be taken as an answer, but to say that it seems to me expedient and proper to wait and allow the thoughts that my papers may raise, to be considered and judged of at their leisure by those who are inclined to review and advance the subject. Perhaps, after a respectful interval, I may be induced to put forth such explanations, acknowledgments, or conclusions, as the state of the subject may then seem to render necessary or useful.

In the meantime, the more we can enlarge the number of anomalous facts and consequences the better it will be for the subject; for they can only remain anomalous to us whilst we continue in error. I may say, however, that the idea you suggest presents no difficulty to me; for having on former occasions (Exp. Res. 2501) had to consider the magne-crystallic phenomena presented by the same body in different media, and having found the magne-crystallic difference unchanged in the media, I have no difficulty in conceiving that a body (as bismuth), which in the amorphous state is of the same magnetic character as the medium around it, shall, when employed as a crystal, be para-magnetic in one direction and diamagnetic in another (3157). What happens in a medium may, according to my knowledge of the facts, happen in space; and is in full accordance with Thomson's clear paper on the theory of magnetic induction in crystalline bodies.†

In respect of the effects of pressure, to which you refer in

* Phil. Mag. 1855, vol. ix. p. 205.
† Phil. Mag. 1851, vol. i. p. 177.

your letter, we cannot easily draw conclusions on either side until we know better what pressure does. I am not aware whether you consider that pressure on bismuth, whilst it makes the metal more diamagnetic in one direction than another, also makes it more diamagnetic as a whole than before; or whether you suppose it *less* diamagnetic in the transverse direction of the pressure than at first. Gmelin says, on the authority of Marchand and Scheerer (vol. iv. p. 428), that the density of bismuth is diminished as pressure upon it is increased, and extraordinary as the fact seems, gives densities of the following degree for increasing pressures, 9·783, 9·779, 9·655, 9·556; a change in texture at the same time occurring. If the statement be true, then the line of pressure in your beautiful experiments may be the line of *least density* or of *least approximation*, though I hardly know how to think so; still it becomes difficult for us to draw reasons from the constitution of a compressed body, until we know what happens during the compression, although no difficulty arises in considering it, after compression in one direction, like to a magne-crystallic substance.

You are aware (and I hope others will remember) that I give the lines of force* only as *representations* of the magnetic power, and do not profess to say to what physical idea they may hereafter point, or into what they will resolve themselves. Advancing no principle, I say, that the hypothetical fundamental ideas already advanced, when taken in relation to the body of facts now known, are self-contradictory and inapplicable. The following points, namely,—that the *direction* and *polarity* of lines of magnetic force are always shown truly by the electric current induced in metal moving within their influence;—that the dualities of electricity and magnetism are always respectively and essentially related;—that the dualities of an isolated magnet are not related back in straight lines through the magnet;—are to my mind not hypothetical in character, but easily provable by experiment:—and they, with the considerations arising from the principle of the conservation of force, seem to me to be left unexplained by, and in opposition to, the usual hypotheses. No difference arises about the laws of magnetic action and their mathematical development; and that, simply because they are

* It is nearly twenty-four years since I first called attention to these lines (Exp. Res. 114, note).

as yet applied only partially, and thus far are in accordance with *all* the views taken, including mine. When the attempt is made to apply them so as to include at once *paramagnetic*, *diamagnetic*, and *electro-magnetic* phenomena, and at the same time to deduce them from *one* hypothetical cause, then they may become so large and yet precise as to enable us to distinguish between true and false assumptions. On my part, I endeavour not to assume anything, but only to draw such conclusions from the assumptions already made, and the phenomena now discovered, as seem subject to experiment and tangible by facts.

Some persons may feel surprised that I dwell upon points which are perfectly and mathematically explained by the hypothesis of two magnetic fluids, as, for instance, places of little or no action (3341, &c.). My reason is, that being satisfied by the phenomena of diamagnetism, &c. that that hypothesis cannot be true, all these and such like phenomena acquire a new character and a high importance which they had not before, and amongst other philosophical uses, point most emphatically to the essential relation to the dualities and their equivalency in power. They do not contradict the old hypothesis when that is partially applied, but they are not the less strong and striking as evidence in favour of the view of lines of force.

I am, my dear Tyndall,
Yours very faithfully,
M. Faraday.

Royal Institution:
March 14, 1855.

[The subject of a Magnetic Medium was further discussed by Professor Williamson, Phil. Mag. vol. ix. p. 541, and by Professor Hirst, Phil. Mag. vol. x. p. 442. I may say that I strongly lean towards the view that the luminiferous ether is concerned in magnetical and electrical phenomena. Some remarks on this subject are made further on.]

IV.—OBSERVATIONS ON THE 'MAGNETIC MEDIUM,' AND ON THE EFFECTS OF COMPRESSION.

By Professor William Thomson.

[I did myself the pleasure of forwarding to Professor, now Sir William Thomson, a copy of my letter to Faraday, and received from him the following observations in reply. They were too interesting to be kept private, and at my request were published in the 'Phil. Mag.' for April, 1855.—J. T., 1870.]

2 College, Glasgow, March 12, 1855.

My dear Sir,—Allow me to thank you for the abstract of your lecture on magnetism, and the copy of your letter to Mr. Faraday, which I have recently received from you, and have read with much interest. I am still strongly disposed to believe in the magnetic character of the medium occupying space, and I am not sure but that your last argument in favour of the reverse bodily polarity of diamagnetics may be turned to support the theory of universally direct polarity. There is no doubt but that the medium occupying interplanetary space, and the best approximations to vacuum which we can make, have perfectly decided mechanical qualities, and among others, that of being able to transmit mechanical energy in enormous quantities (a platinum wire, for instance, kept incandescent by a galvanic current in the receiver of an air-pump, emits to the glass and external bodies the whole mechanical value of the energy of current spent in overcoming its galvanic resistance). Some of these properties differ but little from those of air or oxygen at an ordinary barometric pressure. Why not, then, the magnetic property? (of which we know so little that we have no right to pronounce a negative.) Displace the interplanetary medium by oxygen, and you have a slight increase of magnetic polarity in the locality with a drawing in of the lines

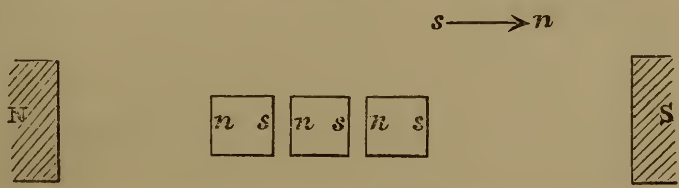
of force. Displace it with a piece of bismuth or a piece of wood, and a slight decrease of magnetic polarity through the locality takes place, accompanied by a pushing out of the lines of force. A state of strain by compression may enhance, in the direction of the strain, that quality of the substance by which it lessens the magnetisability of the space from which it displaces air or 'æther;' just as a similar state may enhance, in the direction of compression, the augmenting power of a paramagnetic substance.

By the bye, a long time ago (rather more than a year after the Edinburgh meeting of the British Association) I repeated with much pleasure some of your compression experiments, and found a piece of fresh bread instantly affected by pressure, so as always to turn the compressed line perpendicular to the lines of force, to whatever form the fragment was reduced. A very slight squeeze between the fingers was quite enough to produce this property, or again to alter it so as to make a new line of compression set equatorially. I repeated it a few days ago with the same results, and got a ball of bismuth, too, to act similarly. I remember formerly finding the bread *attracted* as a whole, instead of being repelled, as I expected from your results. I suppose, however, this must have resulted from some ferruginous impurities, which it may readily have got either in the course of the experiments with it, or in the baking. I mean to try this again.*

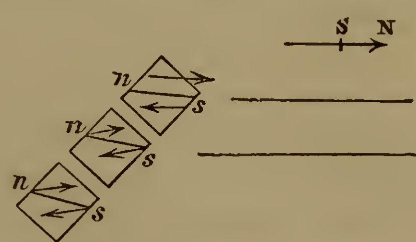
I do not quite admit the argument you draw from your compression experiments regarding the effect of contiguity of particles, because in fact we know nothing of the actual state of the molecules of a strained solid. You have made out a most interesting fact regarding their magnetic bearings; but experiments are neither wanted, nor can be made, to show any sensible effect whatever of the mutual influence of a row of small pieces of bismuth placed near one another, or touching one another. It is perfectly easy to demonstrate that it *must* be such as to impair the 'diamagnetisation' of each piece when the line of the row is parallel to the lines of force, and to

* Professor Thomson's supposition is correct; pure bread is *repelled* by a magnetic pole. I may remark that I am at present engaged in the further examination of the influence of compression, and have already obtained numerous instructive results.—J. T.

enhance it when that line is perpendicular to the lines of force, but in each case to so infinitesimally minute a degree, as to be wholly inappreciable to the most refined tests that have ever been applied. For let the lines of force be parallel to the line shown in the figure, and act on a steel needle in the manner there repre-



sented. Then, whatever hypothesis be true for diamagnetism, there is not a doubt but that each piece is acted on, and consequently reacts, precisely as a piece of steel very feebly magnetised, with its magnetic axis reverse to that of a steel needle free to turn, substituted for it, would do. Each piece of bismuth therefore acts as a little magnet, having its polarity as marked in the diagram, would do. Hence the magnetising force by which the middle fragment is influenced is less than if the two others were away (this being such a force as would be produced by a north pole on the right-hand side of the diagram, and a south pole on the left). It is easily seen, similarly, that if the line joining the centres be perpendicular to the lines of force, the magnetising force on the space occupied by the middle fragment is increased. Corresponding assertions are true for the terminal fragments, although the disturbing effect will be less on them in each case than in the middle one. Hence the diamagnetisation of each will be enfeebled in the former case and enhanced in the latter, by the presence of the others. It follows, according to the principle of superposition of magnetisations, that if the line of the row be placed obliquely across the lines of force, the magnetic axis of each particle, instead of being exactly parallel to the lines of force, will be a little inclined to them, in the angle between their direction and the direction transverse to the bar. The magnets causing the force of the field must act on the little diamagnets, each with its axis thus rendered somewhat oblique, so as to produce on it a statical couple (as shown by the arrow heads), and the re-



sultant of the couples thus acting on the fragments will, when all these are placed on a frame, or rigidly connected, tend to turn the whole mass in such a direction as to place the length of the bar along the lines of force. Still, I repeat, this action, although demonstrated with as much certainty as the parallelogram of forces, is so excessively feeble as to be absolutely inappreciable. A fragment of bismuth, of any shape whatever, held in any position whatever in any kind of magnetic field, uniform or varying most intensely, only exhibits the resultant action of couples on all its small parts if crystalline, and of forces acting always according to Faraday's law on them if the field in which it is placed be non-uniform. Some phenomena that have been observed are to be explained by the resultant of forces from places of stronger to places of weaker intensity in the field, others by the resultant of couples depending on crystalline structure, and others by the resultant of such forces and couples coexisting; and none observed depend at all on any other cause.

I gave a very brief summary of these views (which I had explained somewhat fully and illustrated by experiments on paramagnetics of sufficient inductive capacity to manifest the effects of mutual influence, at the meeting at Belfast) as an abstract of my communication, for publication in the Report of the Belfast meeting of the British Association, where you may see them stated, I hope intelligibly. The experiments on the paramagnetics are very easy, and certainly exhibit some very curious phenomena, illustrative of the resultant effects due to the attractions experienced by the parts in virtue of a variation of the intensity of the field, and to the couples they experience when their axes are diverted from parallelism to the lines of force by mutual influence of the magnetised parts.

I had no intention of entering on this long disquisition when I commenced, but merely wished to try and briefly point out, that the assertions I have made regarding mutual influence are demonstrable in every case without special experiment, are confirmed amply by experiment for paramagnetics, and are absolutely incontrovertible, as well as incapable of verification, by experiment or observation on diamagnetics.

Believe me, yours very truly,

William Thomson.

Prof. Tyndall.

V.—PROFESSOR W. WEBER ON THE THEORY OF DIAMAGNETISM.

[To Professor Wilhelm Weber, who took such a prominent and distinguished part in the discussion of the question of diamagnetic polarity, I forwarded a copy of the Bakerian Lecture for 1855 (p. 89); and also a letter giving a sketch of some experiments executed with the instrument devised for me by M. Weber himself. He favoured me with the following letter, which was published in the 'Phil. Mag.' for December, 1855.—J. T., 1870.]

'Göttingen, September 25, 1855.

'My dear Sir,—Accept my best thanks for your kind communication of September 3; I am gratified to learn that the apparatus executed by M. Leyser in Leipzig for the demonstration of diamagnetic polarity has so completely fulfilled your expectations. This intelligence is all the more agreeable to me, inasmuch as before the apparatus was sent away, it was not in my power to go to Leipzig and test the instrument myself.

'It gave me great pleasure to learn that Mr. Faraday and M. De la Rive have had an opportunity of witnessing the experiments, and of convincing themselves as to the facts of the case.

'It was also of peculiar interest to me to learn that you had succeeded in establishing the polarity of the self-same heavy glass with which Faraday first discovered diamagnetism. This is the best proof that these experiments do not depend upon the conductive power of bismuth for electricity.

'I have read with great interest your memoir "On the Diamagnetic Force," &c. contained in the "Philosophical Transactions," vol. cxlv. It has been your care to separate the *fact* of

diamagnetic polarity from the *theory*, and to place the former beyond the region of doubt. Allow me, with reference to this subject, to direct your attention to a passage at page 39 of your memoir, which you adduce as a conclusion from my theory; the passage runs as follows:—

'"The magnetism of two iron particles in the line of magnetisation is increased by their reciprocal action; but, on the contrary, the diamagnetism of two bismuth particles lying in this direction is diminished by their reciprocal action."

'This proposition is by no means a necessary assumption of my theory, but is rather a direct consequence of diamagnetic polarity, if the facts be such as both you and I affirm them to be. What, therefore, you have adduced against the above conclusion must be regarded as an argument against diamagnetic polarity itself. The *diamagnetic reciprocal action* of the bismuth particles in the line of magnetisation is necessarily opposed to the *action of the exciting magnetic force*. The latter must be enfeebled, because the diamagnetic is opposed to the *magnetic reciprocal action* of iron particles which lie in the line of magnetisation, through which latter it is known the action of the exciting magnetic force is increased. Hence also the *modification* produced in bismuth by magnetic excitement, whatever it may be, must be weakened, because the force of excitation is diminished.

'(I believe, however, that this argument against diamagnetic polarity may also be surmounted. The phenomenon which you have observed must be referred to other circumstances, also connected with the compression of the bismuth. For the diamagnetic reciprocal action is, as I have shown, much too weak to produce an effect which could be compared in point of magnitude with the reciprocal action produced in the case of iron.)

'I take this opportunity of adding a few remarks for the purpose of setting my theory of diamagnetic polarity in a more correct light.

'My theory assumes:—1, that the fact of diamagnetic polarity is granted; 2, that in regard to magnetic phenomena, Poisson's theory of two magnetic fluids, and Ampère's theory of molecular currents, are equally admissible. Whoever denies the first fact, or rejects the theory of Ampère, cannot, I am ready to confess, accept my theory.

'But supposing that you do not reject Ampère's theory of permanent molecular currents, but are disposed to enter upon the inner connection and true significance of the theory, you will easily recognise that it is by no means *an arbitrary assumption of mine*, that in bismuth molecular currents are excited, when the exciting magnetic force is augmented or diminished; but that the excitation of such molecular currents is *a necessary conclusion from the theory of Ampère*, which conclusion Ampère himself could not make, because the laws of voltaic induction, discovered by Faraday, were unknown to him. In all cases where molecular currents *exist*, by increase or diminution of the magnetic exciting force, *molecular currents must be excited*, which either add their action to, or subtract it from, the action of those already present.

'Finally, permit me to make a few remarks on the following words of your memoir :—

'"To carry out the assumption here made, M. Weber is obliged to suppose that the molecules of diamagnetic bodies are surrounded by channels, in which the induced currents, once excited, continue to flow without resistance."

'The assumption of channels which surround the molecules, and in which the electric fluids move without resistance, is an assumption contained in the theory of Ampère, and is by no means added by me for the purpose of explaining diamagnetic polarity. *A permanent molecular current without such a channel involves a manifest contradiction*, according to the law of Ohm.

'I may further observe, that I do not wonder that you regard a theory which is built upon the assumption of such channels, as "so extremely artificial that you imagine the general conviction of its truth cannot be very strong." In a certain sense I quite agree with you, but I only wish to convince you that this objection applies really to the theory of Ampère,* and only applies to mine in so far as it is built upon the former. (You may perhaps find less ground for objecting to the specialty of such an assumption, if you separate the simple fundamental conception, which recommends itself particularly by a certain analogy of the molecules to the heavenly bodies in space, from those additions which Ampère was forced to make, in order to apply the mathematical methods at his command, and to make

* This is quite true.—J. T., 1870.

the subject one of strict calculation. He was necessitated to reduce the case to that of *linear* currents, which necessarily demand channel-shaped bounds, if every possibility of a lateral outspreading is to be avoided.)

'To place my theory of diamagnetic polarity in a truer light, I am anxious also to convince you that this theory is by no means based upon new assumptions (hypotheses), but that it only rests upon such conclusions as may be drawn from the theory of Ampère, when the laws of voltaic induction discovered by Faraday, and the laws of electric currents by Ohm, are suitably connected with it. I affirm, that, even if Faraday had not discovered diamagnetism, by the combination of Ampère's theory with Faraday's laws of voltaic induction, and Ohm's laws of the electric current, as shown in my memoir, the said discovery might possibly have been made.

'In respect, however, to the artificiality of the theory of Ampère, I hope that mathematical methods may be found whereby the limitation before mentioned to the case of linear currents may be set aside, and with it the objection against channel-form beds. All our molecular theories are still very artificial. I, for my part, find less to object to in this respect in the theory of Ampère than in other artificialities of our molecular theories; and for this reason, that in Ampère's case the nature of the artificiality is placed clearly in view, and hence also a way opened towards its removal.*

'To Mr. Faraday I beg of you to present my sincerest respect.

'Believe me, dear Sir,

'Most sincerely yours,

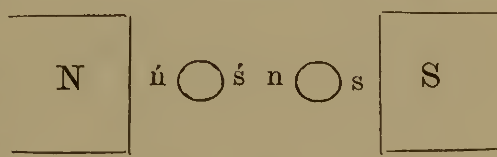
'WILHELM WEBER.'

'Professor Tyndall.'

The foregoing letter possesses more than a private interest, and I have therefore laid it before the readers of the 'Philo-

* In *Heat as a Mode of Motion*, 4th edition, and elsewhere, I write thus:—'Whether we see rightly or wrongly—whether our insight be real or imaginary—it is of the utmost importance in science to aim at perfect clearness in the description of all that comes, or seems to come, within the range of the intellect. For if we are right, clearness of utterance forwards the cause of right; while if we are wrong, it ensures the speedy correction of error.' It is needless to say more to show how heartily I subscribe to the view of Professor Weber.—J. T., 1870.

sophical Magazine.' On one point in it only would I ask permission to make a remark, and that is the proposition, that the diminution of the excitement of a row of bismuth particles in the line of magnetisation by their reciprocal action is '*a direct consequence of diamagnetic polarity.*' M. Weber (I believe) founds this proposition on the following considerations :—Let a series of bismuth particles lie in the axial line between the magnetic poles N and S: the polarity excited in these particles by the direct action of the poles will be that shown in the figure, being the reverse of that of iron particles under the same circumstances. But as the end *n* of the right-hand particle tends to excite a magnetism like its own in the end *s′* of the left-hand particle, and *vice versâ*, this action is opposed to that of the magnet, and hence the magnetism of such a row of particles is enfeebled by their reciprocal action.



Now it appears to me that there is more assumed in this argument than experiment at present can bear out. There are no experimental grounds for the assumption, that what we call the north pole of a bismuth particle exerts upon a second bismuth particle precisely the same action that the north pole of an iron particle would exert. Magnetised iron repels bismuth; but whatever the *fact* may be, the *conclusion* is scarcely warranted, that *therefore* magnetised bismuth will repel bismuth. Supposing it were asserted that magnetised iron attracts iron and repels bismuth, while magnetised bismuth attracts bismuth and repels iron, would there be anything essentially impossible, self-contradictory, or absurd involved in the assertion? I think not. And yet if even the possible correctness of such an assertion be granted, the proposition above referred to becomes untenable. It will be observed that it is against a conclusion rather than a fact that I contend. With regard to the fact, I should be sorry to express a positive opinion; for this is a subject on which I am at present seeking instruction, which may lead me either to M. Weber's view or the opposite. Be that as it may, the result cannot materially affect the respect I entertain for every opinion emanating from my distinguished correspondent on this and all other scientific subjects.

J. T., 1855.

VI.—ON RECIPROCAL MOLECULAR INDUCTION.*

[A letter addressed to Professor W. Thomson.]

Royal Institution, November 26, 1855.

My dear Sir,—The communication from Professor Weber which appears in the present Number of the 'Philosophical Magazine,' has reminded me, almost too late, of your own interesting letter on the same subject published in the April Number of this Journal. A desire to finish all I have to say upon this question at present induces me to make the following remarks, which, had it not been for the circumstance just alluded to, might have been indefinitely deferred.

With reference to the mutual action of a row of bismuth particles, you say that 'it is perfectly easy to demonstrate that it *must* be such as to impair the "diamagnetisation" when the line of the row is parallel to the lines of force.' From this you infer, that in a uniform field of force a bar of bismuth would set its length along the lines of force. Further on it is stated that this action is 'demonstrated with as much certainty as the parallelogram of forces;' and you conclude your letter thus:—'The assertions which I have made are demonstrable in every case without special experiment . . . and are absolutely incontrovertible, as well as incapable of verification, by experiment or observation on diamagnetics.'

Most of what I have to say upon this subject condenses itself into one question.

Supposing a cylinder of bismuth to be placed within a helix, and surrounded by an electric current of sufficient intensity, can you say, *with certainty*, what the action of either end of that cylinder would be on an external fragment of bismuth presented to it?

If you can, I, for my part, shall rejoice to learn the process by which such certainty is attained; but if you cannot, it will, I think, be evident to you that the verb '*must*' is logically 'defective.'

* Phil. Mag., December, 1855.

We *know* that magnetised iron attracts iron: we *know* that magnetised iron repels bismuth: this, so far as I can see, is your only experimental ground for *assuming* that magnetised *bismuth* repels bismuth, and yet you affirm that an action deduced from this assumption 'is demonstrated with as much certainty as the parallelogram of forces.' Do I not state the question fairly? I can, at all events, answer for my earnest wish to do so.

It is needless to remind one so well acquainted as you are with the mental experience of the scientific inquirer, that the very letters which you attach to your sketch, page 291 [223 here], may tempt us to forget the possibility of a physical difference between the *n* of iron and the *n* of bismuth, and may thus lead us very wide of the truth. The very term 'pole' often pledges us to a theoretic conception without our being conscious of it. You are also well aware of the danger of shutting the door against experimental inquiry on an unpromising subject; and when you apparently do this in your concluding paragraph, I simply accept it as a strong way of expressing your personal conviction, that the action referred to is too feeble to be rendered sensible by experiment.

Believe me, dear Sir,

Most truly yours,

JOHN TYNDALL.

Professor W. Thomson, F.R.S.

VII.—PROFESSOR W. THOMSON ON THE RECIPROCAL ACTION OF DIAMAGNETIC PARTICLES.

'Glasgow College, December 24, 1855.

'My dear Sir,—I have been prevented until to-day, by a pressure of business, from replying to the letter you addressed to me in the Number of the "Philosophical Magazine" published at the beginning of this month.

'You ask me the question, "Supposing a cylinder of bismuth to be placed within a helix, and surrounded by an electric current of sufficient intensity, can you say, *with certainty*, what the action of either end of that cylinder would be on an external fragment of bismuth presented to it?"

'In answer, I say that the fragment of bismuth will be *repelled* from either end of the bar, provided the helix be infinitely long, or long enough to exercise no sensible direct magnetic action in the locality of the bismuth fragment. I can only say this with the same kind of confidence that I can say the different parts of the earth's atmosphere attract one another. The confidence amounts in my own mind to a feeling of *certainty*. In every case in which the forces experienced by a little magnetised steel needle held with its axis reverse along the lines of force, and a fragment of bismuth substituted for it in the same locality of a magnetic field, have been compared, they have been found to agree. In a vast variety of cases, a fragment of bismuth has been found to experience the opposite force to that experienced by a little ball of iron, that is, the same force as a little steel magnet held with its axis reverse to the lines of force; and in no case has a discrepance, or have any indications of a discrepance, from this law been observed. I feel therefore in my own mind a certain conviction, that even when the action is so feeble that no force can be discovered at all on the bismuth by experimental tests, such, in regard to sensibility, as have been hitherto applied, the bismuth is really acted on by the

same force as that which a little reverse magnet, if only feeble enough, would experience when substituted in its place. Now there is no doubt of the nature of the force experienced by the steel magnet, or by a little ball of soft iron, in the locality in which you put the fragment of bismuth. One end of a magnetised needle will be attracted, and the other end repelled by the neighbouring end of the bismuth bar; and the attraction or the repulsion will preponderate according as the attracted or the repelled part is nearer. There is then certainly repulsion when the steel magnet is held in the reverse direction to that in which it would settle if balanced on its centre of gravity. In every case in which any magnetic force at all can be observed on a fragment of bismuth, it is such as the steel magnet thus held experiences. Therefore I say it is in this case repulsion. But it will be as much smaller in proportion to the force experienced by the steel magnet, as it would be if an iron wire were substituted for the bismuth core. Yet in this case the repulsion on the bismuth is very slight, barely sensible, or perhaps not at all sensible when the needle exhibits most energetic signs of the forces it experiences. You know yourself, by your own experiments, how very small is even the *directive* agency experienced by a steel magnet placed across the lines of force due to the bismuth core. You may judge how much less sensible would be the attraction or repulsion it would experience as a whole, if held along the lines of force; and then think if the corresponding force experienced by a fragment of bismuth substituted for it is likely to be verified by direct experiment or observation. I think you will admit that it is "incapable of verification," as well as "incontrovertible" by any collation of the results of experiments hitherto made on diamagnetics. As to the concluding paragraph of my letter which you quote, you do me justice when you say you accept it as an expression of my "personal conviction that the action referred to is too feeble to be rendered sensible by experiment." I will not maintain its unqualified application to all that can possibly be done in future in the way of experimental research to test the mutual action of diamagnetics under magnetic influence. On the contrary, I admit that no real physical agency can be rightly said to be "incapable of verification by experiment or observation;" and I will ask you to limit that expression to experiments and

observations hitherto made, and to substitute for the concluding paragraph of my letter the following statement, written for publication three days later, and published in the same Number of the Magazine as that to which you communicated my letter (Phil. Mag. April 1855, p. 247). "The mutual influence" between rows of balls or cubes of bismuth in a magnetic field, "and its effects" in giving a tendency to a bar of the substance to assume a position along the lines of force, "are so excessively minute, that they cannot possibly have been sensibly concerned in any phenomena that have yet been observed; and it is probable that they always remain insensible, even to experiments especially directed to test them."

'I remain, my dear Sir,

'Yours very truly,

'William Thomson.

'Dr. Tyndall.'

VIII.—FARADAY ON MAGNETIC HYPOTHESES.

[A bold and ingenious theory of electro-magnetic action generally, including diamagnetism, has been propounded by De la Rive.* I cannot give a better notion of this theory than by printing here a brief abstract of a Lecture on Magnetic Hypotheses, given by Faraday on June 9, 1854.† He passes in review the various notions entertained regarding electric, chemic, and electro-magnetic phenomena, and adds to them his own view, which really consists in recommending a suspension of judgment until clearer light arrives.]

This discourse, the purpose of which was to direct the attention of the audience to the different hypothetical attempts made to account physically for the known properties of matter in relation to its magneto-electrical phenomena, followed on very naturally to that of Dr. Frankland on the 2nd instant, who then gave an account of the different views advanced by Davy, Ampère, and Berzelius, of the manner in which electricity might be associated with the atoms or molecules of matter, so as to account for their electro-chemical actions, and of the logical and experimental objections which stood in the way of each. On the present occasion reference was first made to Coulomb's investigations of mutual magnetic actions; to the hypothesis advanced by him, that two magnetic fluids, associated with the matter of magnetic bodies, would account for all the phenomena; and to Poisson's profound mathematical investigation of the sufficiency of the hypothesis. Then Oersted's discovery of the relation of common magnetism to currents of electricity was recalled to mind:—hence an enormous enlargement of the scope of magnetic force and of our knowledge of its actions; and hence Ampère's beautiful investigations, and his hypothesis (also sustained by the highest mathematical investi-

* Treatise on Electricity, vol. ii. pp. 48–53 (English Translation).

† Proceedings of Royal Institution, vol. i. p. 457.

gation),—that all magnetic phenomena are due to currents of electricity; and that in such bodies as magnets, iron, nickel, &c. the atoms or particles have naturally currents of electricity running round them in one direction, about what may be considered as their equatorial parts. After Oersted's time, further experimental discoveries occurred; currents of electricity were found competent to induce collateral currents, and magnets proved able to produce like currents; thus showing the identity of action of magnets and currents in producing effects of a kind different from ordinary magnetic attractions and repulsions. Then diamagnetism was discovered, in which actions analogous to those of ordinary magnetism occurred, but with the antithesis of attraction for repulsion and repulsion for attraction: and these were so extensive, that whatever bodies were not magnetic proved to be diamagnetic; and thus *all* matter was brought under the dominion of that magnetic force, whose physical mode of action hypothesis endeavours to account for. As the hypothesis of Ampère could not account for diamagnetic action, some assumed that magnetic and electric force might, in diamagnetic matter, induce currents of electricity in the reverse direction to those in magnetic matter; or else might induce currents where before there were none: whereas in magnetic cases it was supposed they only constrained particle-currents to assume a particular direction, which before were in all directions. Weber stands eminent as a profound mathematician who has confirmed Ampère's investigations as far as they proceeded, and who has made an addition to his hypothetical views; namely, that there is electricity amongst the particles of matter, which is not thrown into the form of a current until the magnetic induction comes upon it, but which then assumes the character of current, having a direction the contrary to that of the currents which Ampère supposed to be always circulating round magnetic matter; and so these other matters are rendered diamagnetic.

De la Rive, who has recently most carefully examined the various hypotheses, and who as an experimentalist and discoverer has the highest right to enter into the consideration of these deep, searching, and difficult inquiries, after recalling the various phenomena which show that the powers concerned belong to the particles of matter and not to the masses merely

(the former conferring them by association upon the latter), then distinguishes magnetic action into four kinds or modes,—namely, the ordinary, the diamagnetic, the induction of currents, and the rotation of a ray; and points out that any acceptable hypotheses ought to account for the *four* modes of action, and, it may be added, ought to agree with, if not account for, the phenomena of electro-chemical action also. De la Rive conceives that as regards these modes of action this hypothetical result may be obtained, and both Ampère's and Weber's views also retained, in the following manner. All the atoms of matter are supposed to be endowed with electrical currents of a like kind, which move about them for ever, without diminution of their force or velocity, being essentially a part of their nature. The direction of these currents for each atom is through one determinate diameter, which may therefore be considered as the axis. Where they emerge from the body of the atom they divide in all directions, and running over every part of the surface converge towards the opposite end of the axis diameter, and there re-enter the atom to run ever through the same course. The converging and diverging points are as it were poles of force. Where the atoms of matter are close or numerous in a given space (and chemical considerations lead to the admission of such cases), the hypothesis then admits that several atoms may conjoin into a ring, so that their central or axial currents may run one into the other, and not return as before over the surface of each atom: these form the molecules of magnetic matter, and represent Ampère's hypothesis of molecular currents. Where the atoms, being fewer in a given space, are farther apart, or where, being good conductors, the current runs as freely over the surface as through the axis, then they do not form like groups to the molecules of magnetic matter, but are still considered subject to a species of induction by the action of external magnets and currents; and so give rise to Weber's reverse currents. The induction of momentary currents and the rotation of a ray are considered by De la Rive as in conformity with such a supposition of the electric state of the atoms and particles of matter.

The Lecturer seemed to think that the great variety of these hypotheses and their rapid succession were rather a proof of weakness in this department of physical knowledge than of

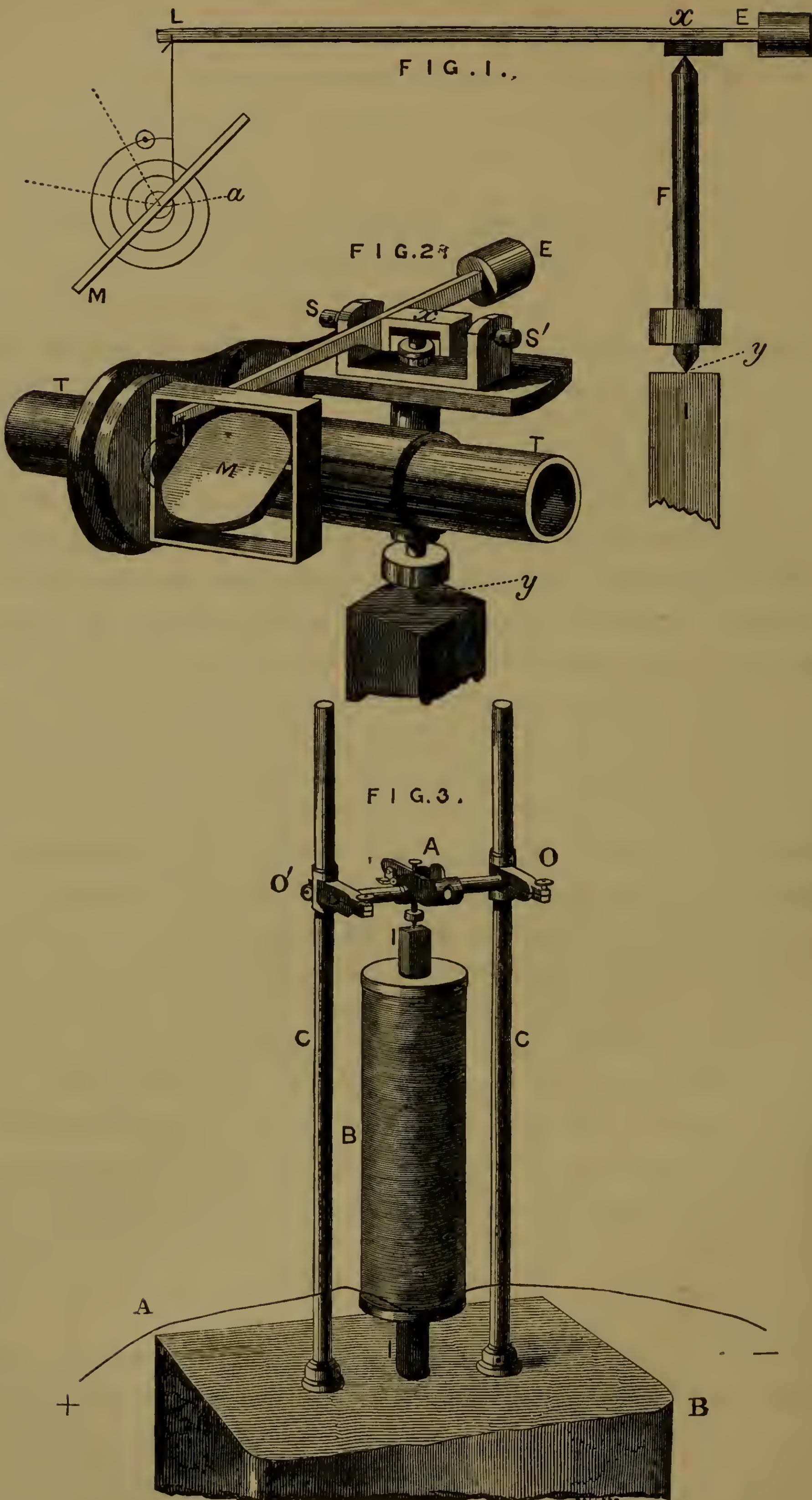
strength, and that the large assumptions which were made in turn for each should ever be present to the mind. Even in the most perfect of them, *i. e.* De la Rive's, these assumptions are very considerable; for it is necessary to conceive of the molecules as being flat or disc-like bodies, however numerous the atoms of each may be; also that the atoms of one molecule do not interfere with or break up the disposition of those of another molecule; also that electro-chemical action may consist with such a constituted molecule; also that the motive force of each atom current is resident in the axis; and on the other hand that the passage of the current over the surface offers *resistance*; for unless there were a difference between the axial and the surface force in one direction or the other, the atoms would have no tendency to congregate in molecules. In making these remarks, however, the speaker had no thought of depreciating hypothesis or objecting to its right use. No discoverer could advance without it; and such exertions as those made by De la Rive, to bring into harmony thoughts which in their earlier forms were adverse to each other, were of the more value, because they were the exertions of a man who knew the value both of hypothesis and of laws, of theory and of fact, and had given proofs of the power of each by the productions of his own mind. Still the speaker advocated that mental reservation which kept hypothesis in its right place and which was ready to abandon it when it failed; and as examples referred to Newton, who (as is shown by his Letters to Bentley) had very strong convictions of the physical nature of the lines of gravitating force, yet in what he publicly advanced stopped short at the law of action of the force, and thence deduced his great results;—and also to Arago, who, discovering the phenomena of magnetic rotation, yet not perceiving their physical cause, had that philosophic power of mind which enabled him to refrain from suggesting one.

IX.—ON SOME MECHANICAL EFFECTS OF MAGNETISATION.

WISHING in 1855 to make the comparison of magnetic and diamagnetic phenomena as thorough as possible, I sought to determine whether the act of magnetisation produces any change of dimensions in the case of bismuth, as it is known to do in the case of iron. The action, if any, was sure to be infinitesimal, and I therefore cast about for a means of magnifying it. The idea which appeared most promising was to augment in the first instance by a lever the small amount of change expected, and to employ the augmented effect to turn the axis of a rotating mirror. By making the axis small enough it was plain that an infinitesimal amount of rectilinear motion might be caused to produce a considerable amount of angular motion. This I proposed to observe by a telescope and scale after the method of Gauss. I consulted Mr. Becker, and, thanks to his great intelligence and refined mechanical skill, I became the possessor of the apparatus now to be described.

A B (fig. 3) is the upper surface of a massive block of Portland stone. It is 21 inches wide, 13 inches deep, and 29 inches high. In it are firmly fixed two cylindrical brass pillars, C C, 1 inch in diameter and 35 inches in height. Over the pillars pass the two clamps, O O′, and from the one to the other passes a cylindrical cross bar, 11 inches long and $\frac{3}{4}$ of an inch wide. This cross bar is capable of two motions; the first up and down the two pillars C C, parallel to itself; the second being a motion round its own axis. To this cross piece is attached the magnifying apparatus A.

The bar to be examined is set upright between the two pillars; being fixed firmly into a leaded screw imbedded in the Portland stone. It is surrounded by an electro-magnetic helix B. On the top of the bar I rests one end of a small cylindrical brass rod, with pointed steel ends. This rod fits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accurately into a brass collar, moving up and down in it with the least possible friction. The other point of the rod presses against a plate of agate very close to a pivot round which the plate can turn. The agate plate is attached to a brass lever 2·1 inches long, whose fulcrum is the pivot just mentioned. Any motion of the point against which the rod presses is magnified about fifty times at the end of the lever. From this end passes a piece of fine steel fibre round the axis of a rotating mirror, which turns as the end of the lever moves. The mirror rotates with its axis. For accurate experiments an illuminated vertical scale is placed at a distance of about twelve feet from the mirror, which is observed through a telescope placed beside the scale. The magnifying apparatus is shown in detail in fig. 2, where M is the mirror; S and S′ two centre-screws, whose points constitute the pivot round which the lever turns; E is a small counterweight; T T is the cross-piece to which the magnifying apparatus is attached. A naked section of the magnifying apparatus is given in fig. 1. I is the bar to be magnetised, F the brass rod with the pointed steel ends, divested of its collar, one of its ends pressing against the plate of agate near the pivot x, and the other resting upon the bar of iron at y. From the end L of the lever the steel fibre passes round the axis a of the mirror M. When the bar I changes its length, the motion at L turns the mirror; and when I resumes its primitive length, the mirror is brought back to its first position by the spiral hair-spring shown in the figure.

In a lecture, of which the following is an abstract, the instrument just described was employed to show the elongation of a bar of iron by magnetism. It is the instrument referred to in 'Heat as a Mode of Motion,' 3rd edition, p. 85. Merely breathing against an iron bar produces a visible expansion. By squirting warm water from a syringe-bottle against the bar, and by employing ether or alcohol in the same way for cooling, the luminous beam which forms the index may, in a few seconds, be caused to pass through a distance of twenty or thirty feet.

On a Magnetic Experiment.*

Some years ago I devised an apparatus to enable me to investigate certain mechanical effects which accompany the act of

* Proceedings of Royal Institution, vol. iv. p. 317.

magnetisation. I wished to apply this apparatus to diamagnetic bodies as well as paramagnetic ones—to bodies such as bismuth, as well as to bodies such as iron. I intend this evening to show you the action of this instrument, and to lay before you some explanation of experiments of which mine are merely confirmatory.

Let us pass quickly in review the excitation of this wonderful power of magnetism. Over the poles of this strong horseshoe Logeman magnet I pass a bent bar of steel, whose arms are the same distance apart as those of the magnet. The steel bar suddenly obtains the power of attracting this iron keeper, and holding it fast. On reversing the stroke of the steel bar its virtue disappears; it is no longer competent to attract the keeper. I continue the stroke of the steel bar in the last direction, and now it is again competent to attract the iron: thus at will we can magnetise and demagnetise this bent piece of steel.

At the other side of the table you observe another mass of metal, bent like the Logeman magnet, but not, like it, naked. This mass, moreover, is not steel, but iron, and it is surrounded by coils of copper wire. At the present moment this huge bent bar is so inert as to be incapable of carrying a single grain of iron. I now send an electric current through the coils that surround it, and its power far transcends that of the steel magnet on the other side. It can carry fifty times the weight. It holds a 56 lb. weight attached to each of its poles, and it empties this large tray of iron nails when they are brought sufficiently near it. On interrupting the current, the power vanishes, and the nails fall.

Now the magnetised iron cannot be in all respects the same as the unmagnetised iron. Some change must take place among the molecules of the iron bar at the moment of magnetisation. And one curious action which accompanies the act of magnetisation I will now try to make sensible to you. Other men have laboured, and we are here entering into their labours. The effect I wish to make manifest was discovered by Mr. Joule, and was subsequently examined by MM. De la Rive, Wertheim, Marian, Matteucci, and Wartmann. It is this. At the moment when the current passes through the coil surrounding the electromagnet, a clink is heard emanating from the body of the iron, and at the moment the current ceases a clink is also heard. In

fact, the acts of magnetisation and demagnetisation so stir the particles of the magnetised body that they, in their turn, can stir the air and send sonorous impulses to our auditory nerves.*

The sounds occur at the moment of magnetisation, and at the moment when magnetisation ceases; hence, if a means be devised of making and breaking, in quick succession, the circuit through which the current flows, we shall obtain an equally quick succession of sounds. I do this by means of a contact-breaker which belongs to a Ruhmkorff's induction coil. A thin bar of iron stretches from one of the bridges of this monochord to the other. This bar is placed in a glass tube, which is surrounded by copper wire. The contact breaker is placed in a distant room, so that you cannot hear its noise. The current is now active, and every individual in this large assembly hears something between a dry crackle and a musical sound issuing from the bar in consequence of its successive magnetisation and demagnetisation.

Hitherto we have occupied ourselves with the iron which has been acted upon by the current. Let us now devote a moment's time to the examination of the current itself. This naked copper wire is quite unable to attract these iron filings; but I send a voltaic current through it; and now it grapples with the filings, and holds them round it in a thick envelope. I interrupt the current, and the filings fall. Here, also, is a compact coil of copper wire which is overspun with cotton to prevent contact between the convolutions. On sending a current through the coil, a power of attraction is instantly developed, which enables it to empty this plate of iron nails.

Thus we have magnetic action exhibited by a body which does not contain a particle of the so-called magnetic metals. The copper wire is made magnetic by the electric current. Indeed, by means of a copper wire through which a current flows, we may obtain all the effects of magnetism. A long coil is suspended before you so as to be capable of free motion in a horizontal direction: it can move all round in a circle like an ordinary magnetic needle. At its ends I have placed two spirals of platinum wire which the current will raise to brilliant incandescence. They are glowing now, and the suspended coil

* The sound, I find, was first noticed by Mr. Page.—J. T., 16th June.

behaves in all respects like a magnetic needle. Its two ends show opposite polarities: it can be attracted and repelled by a magnet, or by a current flowing through another coil; and it is so sensitive that the action of the earth itself is capable of setting it north and south.

There is an irresistible tendency to unification in the human mind; and, in accordance with our mental constitution, we desire to reduce phenomena which are so much alike to a common cause. Hence the conception of the celebrated Ampère that a magnet is simply an assemblage of electric currents. Round the atoms of a magnet Ampère supposed minute currents to circulate incessantly in parallel planes; round the atoms of common iron he also supposed them to circulate, but in all directions—thus neutralising each other. The act of magnetisation he supposed to consist in the rendering of the molecular currents parallel to a common plane, as they are supposed to be in a permanent magnet.

This is the celebrated theory of molecular currents propounded by Ampère. You observe it consists in the application of conceptions obtained from sensible masses of matter to insensible or atomic masses. Let us follow out this conception to what would appear its legitimate consequences. I have said that we obtain both attractions and repulsions from electric currents: all these effects are deduced from one law, which is, *that electric currents flowing in the same direction attract each other, while, when they flow in opposite directions, they repel each other*. Let me illustrate this law rapidly. Before you are two flat coils facing each other, and about eight inches apart. I send a current through both in the same direction; the coils instantly clash and cling together in virtue of their mutual attraction. I now reverse the current through one of them, and they fly a yard asunder, in virtue of their mutual repulsion. And now one of them twists its suspending wire so as to turn its opposite face to the other coil; the currents are now again in the same direction, and the coils clash and cling as in the first instance. Imagine, then, our molecular currents flowing round the atoms of this iron bar in planes perpendicular to the length of the bar. From the law just enunciated we should infer the mutual attraction of those currents; and from this attraction we should be disposed to infer the *shortening* of the

bar at the moment of magnetisation. Here, for example, is a coil of copper wire suspended vertically; the end of the coil dips into this little basin of mercury. From a small voltaic battery behind I send a current through the coil; and, because it passes in the same direction through all its convolutions, they attract each other. The coil is thereby shortened; its end quits the mercury with a spark; the current ceases; the wire falls by its own gravity; the current again passes, and the wire shortens as before. Thus you have this quick succession of brilliant* sparks produced by the shortening of the wire and the interruption of the current as it quits the mercury.

Is it a fact, then, that an iron bar is *shortened* by the act of magnetisation? It is not. And here, as before, we enter into the labours of other men.

Mr. Joule was the first to prove that the bar is *lengthened.* Mr. Joule rendered this lengthening visible by means of a system of levers and a microscope, through which a single observer saw the action. The experiment has never, I believe, been made before a public audience; but the instrument referred to at the commencement of this lecture will, I think, enable me to render this effect of magnetisation visible to everybody present.

Before you is an upright iron bar, two feet long, firmly screwed into a solid block of wood.† Sliding on two upright brass pillars is a portion of the instrument which you see above the iron bar. The essential parts of this portion of the apparatus are, first, a vertical rod of brass, which moves freely and accurately in a long brass collar. The lower end of the brass rod rests upon the upper flat surface of the iron bar. To the top of the brass rod is attached a point of steel; and this point now presses against a plate of agate, near a pivot which forms the fulcrum of a lever. The distant end of the lever is connected, by a very fine wire, with an axis on which is fixed a small circular mirror. If the steel point be pushed up against the agate plate, the end of the lever is raised; the axis is thereby caused to turn, and the mirror rotates. I now cast a

* Rendered brilliant by the introduction of a coil of wire and a core of soft iron into the circuit.

† The wood was employed merely for lecture-room purposes; for accurate observations the iron bar was always fixed upon the block of Portland stone.—J. T., 1870.

beam from an electric lamp upon this mirror; it is reflected in a luminous sheaf, fifteen or sixteen feet long, and it strikes our screen, there forming a circular patch of brilliant light. This beam is to be our index; it will move as the mirror moves, only with twice its angular velocity; and the motion of the patch of light will inform us of the lengthening and shortening of the iron bar.

I employ two batteries, one to ignite the lamp, and the other to magnetise the iron bar. At the present no current is passing. Let us make the circuit; the bright image on the screen is suddenly displaced. It moves through the distance of a foot. I break the circuit: the bar instantly shrinks to its normal length, and the image returns to its first position. However often you make the experiment, the result is the same. When the bar is magnetised, the image always descends, which declares the lengthening of the bar. When the current is interrupted, the image immediately rises. This is the first time that this action of magnetism has been seen by a public audience.

The same apparatus has been employed in the examination of bismuth bars; and, though considerable power has been applied, I have hitherto failed to produce any sensible effect. It was at least conceivable that complementary effects might be here exhibited, and a new antithesis thus established between magnetism and diamagnetism.

And now for the explanation of this action. I place this large flat magnet upon the table; over it a paper screen; and on the screen I shake iron filings. You know the beautiful lines in which those filings arrange themselves—lines which have become classical from the use made of them in this Institution; for they have been guiding-threads for Faraday's intelligence while exploring the most profound and intricate phenomena of magnetism. These lines indicate the direction in which a small magnetic needle sets itself when placed on any of them. The needle will always be a tangent to the magnetic curve. A little rod of iron, freely suspended, behaves exactly like the needle, and sets its longest dimension in the direction of the magnetic curve. In fact, the particles of iron filings themselves are virtually so many little rods of iron, which, when they are released from the friction of the screen

by tapping, set their longest dimensions along the lines of force. Now, in this bar magnet the lines of force run *along the magnet itself*, and, were its particles capable of free motion, they also would set their longest dimensions parallel to the lines of force —that is to say, parallel to the length of the magnet. This, then, is the explanation given by M. De la Rive of the lengthening of the bar. The bar is composed of irregular crystalline granules; and, when magnetised, these granules tend to set their longest dimensions parallel to the axis of the bar. They succeed, partially, and produce a microscopic lengthening of the bar, which, suitably magnified, has been rendered visible to you. The explanation seems to me as satisfactory as it is acute.

Let me now endeavour to render these beautiful magnetic curves visible to you all. From an electric lamp turned on its back, a vertical cylinder of light issues. Over the aperture of the lamp are placed two small bar magnets, enclosed between two plates of glass. The vertical beam is received upon a looking-glass which reflects it on to the screen. In the path of this reflected beam is placed a lens, which projects upon the screen a magnified image of the two small magnets. And now I sprinkle fine iron sand on the plate of glass, and you see how it arranges itself under the operation of the magnets. A most beautiful display of the magnetic curves is now before you. And you observe, when I tap the glass, how the particles attach themselves by their ends, and how the curves close in upon each other. In the solid iron bar they also try to attach themselves thus, and close thus up; the consequence is that the longitudinal expansion is exactly counterbalanced by the transverse contraction, so that the *volume* of the bar remains unchanged.

But can we not bring a body with movable particles within an electro-magnetic coil? We can; and I will now, in conclusion, show you an experiment devised by Mr. Grove, which bears directly upon this question, but the sight of which, I believe, has hitherto been confined to Mr. Grove himself. At all events, I am not aware of its ever having been made before a large audience. This cylinder with glass ends contains a muddy liquid; the muddiness being produced by the magnetic oxide of iron which is suspended mechanically in water. Round

the glass cylinder are coiled five or six layers of covered copper wire; and here is a battery from which a current can be sent through the coil. First of all, I place the glass cylinder in the path of the beam from our electric lamp, and, by means of a lens, cast a magnified image of the end of the cylinder on the screen. That image at present possesses but feeble illumination. The light is almost extinguished by the suspended particles of magnetic oxide. But, if what has been stated regarding the lines of force through the bar of magnetised iron be correct, the particles of the oxide will suddenly set their longest dimensions parallel to the axis of the cylinder, and also in part set themselves end to end when the current is sent round them. More light will be thus enabled to pass; and now you observe the effect. The moment the circuit is established the disc upon the screen becomes luminous. When the current is interrupted, gloom supervenes; I re-establish it, and we have a luminous disc once more.

The apparatus before you was, as stated, really invented to examine whether any mechanical effect of this kind could be detected in diamagnetic bodies; but hitherto without result. And this leads me to remark on the large ratio which the failures of an original inquirer bear to his successes. The public see the success—the failure is known to the inquirer alone. The encouragement of his fellow-men, it is true, often cheers the investigator and strengthens his heart; but his main trials occur when there is no one near to cheer him, and when, if he works aright, he must work for duty and not for reputation. And this is the spirit in which work has been executed in this Institution, by a man who has, throughout his life, turned a deaf ear to such allurements as this age places within the reach of scientific renown; and it behoves every friend of this Institution to join in the wish that *that* man's spirit may continue to live within its walls, and that those who come after him may not shrink from his self-denial should they ever hope to merit a portion of his fame.

Biot found it impossible to work at his experiments on sound during the day in Paris; he was obliged to wait for the stillness of night. I found it almost equally difficult to make accurate experiments, requiring the telescope and scale, with the

instrument just described in London. Take a single experiment in illustration. The mirror was fixed so as to cause the cross-hair of the telescope to cut the number 727 on the scale; a cab passed while I was observing—the mirror quivered, obliterating the distinctness of the figure, and the scale slid apparently through the field of view and became stationary at 694. I went upstairs for a book; a cab passed, and on my return I found the cross-hair at 686. A heavy waggon then passed, and shook the scale down to 420. Several carriages passed subsequently; the figure on the scale was afterwards 350. In fact, so sensitive is the instrument that long before the sound of a cab is heard its approach is heralded by the quivering of the figures on the scale.

Various alterations which were suggested by the experiments were carried out by Mr. Becker, and the longer I worked with it the more mastery I obtained over it; but I did not work with it sufficiently long to perfect its arrangement. Some of the results, however, may be stated here.

At the beginning of a series of experiments the scale was properly fixed, and the pressure of the pointed vertical rod F, fig. 1, on the end of the iron bar, I, so regulated as to give the mirror a convenient position; then, before the bar was magnetised, the figure cut by the cross-hair of the telescope was read off. The circuit was then established, and a new number, depending on the altered length of the bar by its magnetisation, started into view. Then the circuit was interrupted, and the return of the mirror towards its primitive position was observed. The mirror, as stated, was drawn back to its first position by the spiral hair-spring shown in fig. 1. Here are some of the results :—

	Figure of scale.
Bar unmagnetised	577
,, magnetised	470
,, unmagnetised	517

Here the magnetisation of the bar produced an elongation expressed by 107 divisions of the scale, while the interruption of the circuit produced only a shrinking of 47 divisions. There was a tendency on the part of the bar, or of the mirror, to persist in the condition superinduced by the magnetism. The passing of a cab in this instance caused the scale to move from 517 to 534—that is, it made the shrinking 64 instead of 47. Tapping the bar produced the same effect.

The bar employed here was a wrought iron square core, 1·2 inch a side and 2 feet long.

The following tables will sufficiently illustrate the performance of the instrument in its present condition. In each case are given the figures observed before closing, after closing, and after interrupting the circuit. Attached to each table, also, are the lengthening produced by magnetising and the shortening consequent on the interruption of the circuit:—

Circuit.	Scale 10 cells.		Circuit.	Scale 20 cells.	
Open . .	647		Open . .	653	
Closed . .	516	131 elongation.	Closed . .	475	188 elongation.
Broken . .	581	65 return.	Broken . .	579	114 return.
Open . .	637		Open . .	638	
Closed . .	509	128 elongation.	Closed . .	452	186 elongation.
Broken . .	579	70 return.	Broken . .	568	116 return.
Open . .	632		Open . .	632	
Closed . .	491	141 elongation.	Closed . .	472	160 elongation.
Broken . .	568	77 return.	Broken . .	561	89 return.

These constitute but a small fraction of the number of experiments actually made. There are very decided indications that the amount of elongation depends on the molecular condition of the bar. For example, a bar taken from a mass used in the manufacture of a great gun at the Mersey Iron-works suffered changes on magnetisation and demagnetisation considerably less than those recorded here. I hope to return to the subject.*

* I owe these bars to the liberality of the proprietors of the Mersey Iron-works, through the friendly intervention of Mr. Mallet.

X.—ON THE INFLUENCE OF MATERIAL AGGREGATION UPON THE MANIFESTATIONS OF FORCE.*

[The following four essays are, for the most part, as stated in the note at the commencement, lecture-room summaries of the memoirs already presented to the reader. They, however, contain additional remarks and experiments which may be useful to the scientific teacher. To one of them are appended drawings, hitherto unpublished, of the moulds employed for compressing bismuth in the diamagnetic experiments.

The first of the series is a report of the first lecture delivered by me in the Royal Institution. It was given on Friday evening, February 11, 1853.]

The system of the universe embraces two things,—an object acted upon, and an agent by which it is acted upon;—the object we call matter, and the agent we call force. Matter, in certain aspects, may be regarded as the vehicle of force; thus the luminiferous ether is the vehicle or medium by which the pulsations of the sun are transmitted to our organs of vision. Or to take a plainer case; if we set a number of billiard balls in a row and impart a shock to one end of the series, in the direction of its length, we know what takes place; the last ball will fly away, the intervening balls having served for the transmission of the shock from one end of the series to the other. Or we might refer to the conduction of heat. If, for example, it be required to transmit heat from the fire to a point at some distance from the fire, this may be effected by means of a conducting body—by the poker for instance: thrusting one end of the poker into the fire it becomes heated, the heat makes its way through the mass, and finally manifests itself at the other end. Let us endeavour to get a distinct idea of what we here call heat; let us first picture it to ourselves as an agent apart from the mass of the conductor, making its way among the particles of the latter, jumping from atom to atom,

* Proceedings of the Royal Institution, vol. i. p. 254.

and thus converting them into a kind of stepping-stones to assist its progress. It is a probable conclusion, even had we not a single experiment to support it, that the mode of transmission must, in some measure, depend upon the manner in which those little molecular stepping-stones are arranged. But we need not confine ourselves to the material theory of heat. Assuming the hypothesis which is now gaining ground, that heat, instead of being an agent apart from ordinary matter, consists in a motion of the material particles, the conclusion is equally probable that the transmission of the motion must be influenced by the manner in which the particles are arranged. Does experimental science furnish us with any corroboration of this inference? It does. More than twenty years ago MM. De la Rive and De Candolle proved that heat is transmitted through wood with a velocity almost twice as great along the fibre as across it. Wood has been recently proved by myself to possess three axes of calorific conduction; the first and greatest axis being parallel to the fibre; the second axis perpendicular to the fibre and to the ligneous layers; while the third axis, which marks the direction in which the greatest resistance is offered to the passage of the heat, is perpendicular to the fibre and parallel to the layers.

But it is the modification of the magnetic force by the peculiarities of aggregation, which forms the subject of this evening's discourse. What has been stated regarding heat applies with equal force to magnetism. The action of a magnetic mass is the resultant action of its molecules, and it will be influenced by the manner in which they are aggregated. The fundamental phenomena of magnetism are too well known to render it necessary to dwell upon them for an instant. A small bar of iron was suspended in the magnetic field; it set its length parallel to the line joining the poles. Should we be justified from this experiment in concluding that a magnetic mass will always set its longest dimension axial? No. A second magnetic bar, equal in size to the former, was suspended between the poles; it set its length at right angles to the line joining the poles. Whence this deportment? We find the reason of it in the mechanical structure of the bar: it is composed of magnetic plates, transverse to its length: these plates set from pole to pole and hence the length of the bar sets equatorial. But let

us proceed from this coarse experiment to one more delicate, where nature herself has imposed the conditions of aggregation. A plate taken from a mass of shale, picked up a few weeks ago in the coal district of Blackburn, was suspended between the poles. Though strongly magnetic it set its longest dimension at right angles to the line joining the poles.* This deportment was at once explained by reference to the structure of the mass: it also, though apparently compact, was composed of layers transverse to its length; these layers set from pole to pole and hence the length equatorial. Let us ascend to a case still more refined. A crystal of sulphate of nickel was suspended between the poles, and on exciting the magnet a certain determinate position was taken up by the crystal. The substance was magnetic, still its shortest dimension set from pole to pole. The crystal was removed from the magnetic field, and the edge of a penknife was placed along the line which set axial; a slight pressure split the crystal and disclosed two smooth surfaces of cleavage. The crystal could in this way be cloven into an indefinite number of magnetic layers; these layers set from pole to pole and hence the longest dimension, which was perpendicular to the layers, equatorial. Comparing all these experiments,—ascending from the gross case where the laminæ were plates of iron stuck together by wax, to that in which they were crystalline, the inference appears unavoidable that the unanimity of deportment exhibited is the product of a common cause; and that the results are due to the peculiarities of material aggregation.

The beautiful researches of Plücker in this domain of science are well known. Plücker's first experiment was made with a plate of tourmaline. Suspended in the magnetic field with the axis of the crystal vertical, it set its length from pole to pole, like an ordinary magnetic body. Suspended with the axis of the crystal horizontal, on exciting the magnet, the longest dimension set equatorial. Let us see whether we cannot obtain this deportment in another way. Suspending the piece of shale already made use of, so that its laminæ were horizontal, on exciting the magnet the longest horizontal dimen-

* This is one of the anomalies which require clearing up, for this shale was probably squeezed at right angles to the cleavage; still the laminæ, and not the direction of compression, set from pole to pole.—J. T., 1870.

sion of the plate set axial: moving the point of suspension 90° so that the laminæ were vertical, on exciting the magnet the length of the plate set equatorial. In the magnetic field the deportment of the crystal was perfectly undistinguishable from that of the shale.

But it may be retorted that tourmaline possesses no such laminæ as those possessed by the shale: true—nor is it necessary that it should do so. A number of plates, bars, and disks, formed artificially from magnetic dust, exhibited a deportment precisely similar to the tourmaline,—suspended from one point they set their lengths axial, suspended from another point the lengths set equatorial. Let us now turn to what may be called the complementary actions exhibited by diamagnetic bodies. A homogeneous diamagnetic bar sets its length equatorial. But bars were exhibited composed of transverse diamagnetic laminæ which set their lengths axial. This experiment is complementary to that of the shale, &c.; the magnetic laminæ set axial, the diamagnetic equatorial; and by attention to this the magnetic body is made to behave like a homogeneous diamagnetic body, and the diamagnetic body like a homogeneous magnetic body. Diamagnetic bars and disks were also examined, and a deportment precisely complementary to that of the magnetic bars and disks was exhibited. A magnetic disk set its thickness from pole to pole and consequently its longest dimension equatorial; a diamagnetic disk set its thickness equatorial and its longest dimension from pole to pole. Two bodies of the same exterior form and of the same colour, were suspended simultaneously in the fields of two electro-magnets, and both the latter were excited by the same current; the eye could detect no difference of deportment. Both bodies possessed the shape of a calcite crystal, and both set the crystallographic axis equatorial. One body, however, was composed of wax, while the other was a true crystal. In the same way a crystal of carbonate of iron exhibited a deportment precisely the same as that of a model formed of magnetic [carbonate of iron] dust.

The explanation of these phenomena may be given in a few words. In the construction of the models, the magnetic or diamagnetic dust was formed into a kind of dough and pressed between two glass plates; the same process was ap-

plied to the wax; and it is a general law, that in diamagnetic bodies the line along which the density of the mass has been increased by compression, sets equatorial, and in magnetic bodies axial. A reference to this principle will instantly render plain all the experiments we have described. In those cases where the same artificial bar set at one time axial and at another time equatorial, the deportment depended on the circumstance whether the line of compression was vertical or horizontal. When vertical its directive power was annulled, and the action was determined by the exterior form of the body; but when horizontal its directive action came into play and determined the position of the mass. Our magnetic bar, for example, suspended with its line of pressure vertical, set axial, but with its line of pressure horizontal, it set equatorial; because the pressure had been exerted at right angles to its length. This action is so general that it is difficult to find a body so perfectly homogeneous as not to exhibit it in some degree. Ipecacuanha lozenges and Carlisle biscuits were suspended in the magnetic field and exhibited a most striking directive action. The materials in both cases were diamagnetic; but owing to the pressure exerted in their formation their largest horizontal dimensions set from pole to pole, the line of compression being equatorial.

Let us endeavour to arrive at the precise logical import of these experiments. Let us suppose that before ever a crystal had been suspended in the magnetic field, we were acquainted with the fact that a slight change of density in any direction is accompanied by such modifications of the magnetic force as those above described:—that we knew that flour, bran, soap, shale, magnetic dust, diamagnetic dust, &c., all exhibited this directive action,—that it is in fact a law of matter in general; and then let us imagine some fortunate experimenter hanging a crystal between the poles and observing a deportment in every respect similar. Would not the analogy of the case at once flash upon him? Would he not regard this deportment as a beautiful, but still special example of that all-pervading law with which he was previously acquainted? Would he not congratulate himself on the possibility thus opened to him of searching out the mysteries of crystalline structure, and rendering apparent to his mental eye the manner in which the

molecules are aggregated together? He would never assume the existence of forces altogether new to account for the observed actions; much less would he affirm that they were wholly independent of magnetism or diamagnetism; for he would know beforehand the modification of these forces by the peculiarities of aggregation to be competent to produce the phenomena. But magne-crystallic action was discovered when its universality was unknown; and hence its discoverer was led to regard it as something unique. A great temptation lay in his way: years before a magnet, now present, had twisted a ray of light, and thus suggested a connection between light and magnetism. What wonder then if this unifying instinct, this yearning to find the bond which unites these agents, this prediction of the human mind that all the forces of nature are but branches of a common root,—what wonder, I say, if it jumped its bounds and cried 'I have it!' too soon? For a long time the optic axis, and it alone, was chargeable with these phenomena, which it was now hoped there would be little difficulty in referring to their proper cause, and regarding as examples of the modification of force by the peculiarities of molecular aggregation.

The Lecturer then pointed out the bearing of the described results upon the problem of the diurnal range of the magnetic needle. Professor Faraday had referred the variation of the declination to the modification of atmospheric magnetism by the sun's rays. That an effect was produced here could not for a moment be doubted, but the precise extent of this effect was still an open question. The discovery of a decennial period by Lamont threw a great difficulty in the way of any theory which would refer the diurnal range to thermic action; and the difficulty was greatly increased by the observation of Colonel Sabine, who connected Lamont's discovery with that of Schwabe regarding the solar spots. But whatever the results of future inquiries as to the direct magnetism of the sun may be, no theory which proposes to exhaust the subject can afford to omit the mediate operation of the sun by his heat; not however confining it to the atmosphere, but extending it also to the earth's solid crust. Let us look once more to our experiments. The line of greatest density is that of strongest magnetic power. The body operated upon by the magnet is itself a magnet, and it is an experimental fact, that it is a stronger magnet along the line of greater density than along any

other line. If instead of increasing the density in one direction we increase it in all directions, we thereby augment the general magnetic power of the body.* Anything therefore which tends to increase density increases magnetic power; and whatever diminishes density diminishes magnetic power also. Knowing this, the conclusion is inevitable, that the local action of the sun upon the earth's crust must influence, in some degree, the resultant effect. The action here meant is wholly different from the generation of thermo-electric currents which affect the needle. The simple mechanical change of density is what is meant. It is a true cause, and no complete theory can omit taking it into account.

The Lecturer then proceeded to remark on the influence of geologic changes upon the earth as a magnet, and concluded as follows:

'This evening's discourse is, in some measure, connected with this locality; and thinking thus, I am led to enquire wherein the true value of a scientific discovery consists. Not in its immediate results alone, but in the prospect which it opens to intellectual activity, in the hopes which it excites, in the vigour which it awakens. The discovery which led to the results brought before you to night was of this character. *That* magnet † was the physical birthplace of these results; and if they possess any value they are to be regarded as the returning crumbs of that bread which in 1846 was cast so liberally upon the waters. I rejoice in the opportunity here afforded me of offering my tribute to the greatest worker of the age, and of laying some of the blossoms of that prolific tree which he planted, at the feet of the discoverer of diamagnetism.'

* Some time subsequent to this discourse, I tried in vain to augment sensibly the density of bismuth by pressure.—J. T., 1870.

† The instrument with which Faraday first produced the rotation of the plane of polarisation lay on the table.

XI.—ON DIAMAGNETIC REPULSION.*

It was stated at the commencement of the discourse that bodies are repelled by the poles of a magnet, in virtue of a state of excitement into which they are thrown by the latter. The repulsion of bismuth, and the attraction of soft iron, followed precisely the same laws when the strength of the influencing magnet was augmented, the respective forces being proportional, not simply to the strength, but, within wide limits, to the square of the strength of the magnet. The result is explained in the case of iron by the fact of its being converted, while under magnetic influence, into a true temporary magnet, whose power varies with that of the influencing one; and in the case of bismuth, the result can only be explained by the fact that the diamagnetic mass is converted into a true *diamagnet.*

It was next shown that the condition of excitement evoked by a magnetic pole was not the same as that evoked by another pole of an opposite quality. If the repulsion were independent of the quality of the pole, then two poles of unlike names ought to repel the bismuth, when brought to act upon it simultaneously. This is not the case. Two poles of the same name produce repulsion; but when they are of equal powers and opposite names, the condition excited by one of them, as shown by Reich, is neutralised by the other, and no repulsion follows.

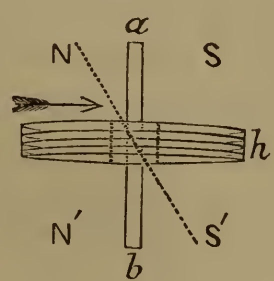
Bars of magnetic and diamagnetic bodies were next submitted to all the forces capable of acting upon them magnetically; first, to the magnet alone; secondly, to the electric current alone; and thirdly, to the magnet and current combined. Attention to structure was here found very necessary, and the neglect of it appears to have introduced much error into this portion of science. Powdered bismuth, without the admixture of any foreign ingredient, was placed in a strong metallic mould,

* Proceedings of the Royal Institution, vol. ii. p. 13.

and submitted to the action of a hydraulic press; perfectly compact metallic masses were thus procured, which, suspended in the magnetic field with the line of compression horizontal, behaved exactly like magnetic bodies, setting their longest dimensions from pole to pole. This identity of deportment with an ordinary magnetic substance was also exhibited in the case of the current singly, and of the current and the magnet combined. In like manner, by the compression of a magnetic powder, magnetic bars were produced, which, between the two poles of a magnet, set exactly like ordinary diamagnetic ones; this identity of deportment is preserved when the bars are submitted to the action of the current singly, and of the current and magnet combined. Calling those bars which show the ordinary magnetic and diamagnetic action *normal bars*, and calling the compressed bars *abnormal ones*, the law follows, that an abnormal bar of one class of bodies exhibits precisely the same deportment, in all cases, as the normal bar of the other class; but when we compare normal bars of both classes together, or abnormal bars of both classes, then the antithesis of action is perfect. The experiments prove that, if that which Gauss calls the *ideal distribution* of magnetism in magnetic bars be inverted, we have a distribution which will produce all the phenomena of diamagnetic ones.

The important question of diamagnetic polarity was submitted to further and stricter examination. A flat helix, whose length was an inch, internal diameter an inch, and external diameter seven inches, was attached firmly to a table with its coils vertical. A mode of suspension was arranged in which a bar of bismuth, five inches long, and 0·4 of an inch in diameter, was permitted to swing freely, while surrounded by the helix. With this arrangement, the following experiments were made:—1. A voltaic current from twenty of Grove's cells was sent through the helix *h*, the direction of the current *in the upper half* of the helix being that denoted by the arrow (fig. 1). The north pole of a magnet being placed at N, the end *a* of the suspended bar, *a b*, of bismuth was *attracted* towards the pole N. 2. The south pole of a second

Fig. 1.



magnet being placed at S, and the current being sent through the helix in the same direction as before, the bar left its central position and approached N with greater force than in the former experiment. The reason was manifest: the state of excitement which causes *a* to be attracted by N causes it to be repelled by S; both poles, therefore, act in unison, and a deflection of greater energy is produced. 3. The pole S being removed to the position S′, the deflection was also found to be about twice as forcible as when the single pole N was employed. Here also the reason is plain: the two ends, *a* and *b*, of the bismuth bar, are in different states of excitement; the end *a* is attracted by a north pole, the end *b* is attracted by a south pole: both poles act therefore as a mechanical couple upon the bar, and produce the deflection observed. 4. The pole S′ was replaced by a north pole of the same strength, thus bringing two poles of the same name to bear upon the two ends of the bar: there was no deflection by this arrangement. Here N's attraction for the end *a* was nullified by the repulsion of the end *b* by a like pole; the experiment thus furnishes an additional proof of the polar condition of *a b*. 5. We have supposed the pole S to be removed into the position S′; but permitting the pole S to remain, and introducing another pole (a south one) at S′, a greater action than that produced with two magnets was obtained. 6. Finally, adding another north pole at N′, and allowing four magnets to operate upon the bismuth bar simultaneously, a maximum action was obtained, and the bar was attracted and repelled with the greatest promptness and decision.

In all these cases where an iron bar was substituted for the bismuth bar a b, *a deflection precisely the opposite to that exhibited by* a b *was produced.* A branch of the current by which the bar of bismuth was surrounded could be suffered to circulate round a bar of iron, suspended freely in an adjacent helix; when the forces acting upon the iron were the same as those acting upon the bismuth, the bars were always deflected in opposite directions.

The question of diamagnetic polarity was next submitted to a test which brought it under the dominion of the principles of mechanics. A movable pole was chosen, of such a shape that the diminution of the force, as the distance was augmented,

was very slow; the field of force being therefore very uniform. Let the space in front of the pole P (fig. 2) be such a field. A *normal* bar of bismuth, $a\ b$, was attached to the end of a lever transverse to the length of the latter, and counterpoised by a weight at the other extremity: the system was then suspended from its centre of gravity g, so that the beam and bar swung horizontally. Supposing the bar to occupy the position shown in the figure, then if the force acting upon it *be purely repulsive*—that is to say, if the diamagnetic force be unpolar—it is evident that the tendency of the force acting upon every particle of the mass of bismuth tends to turn the lever round its axis of suspension, in the direction of the curved arrow. On exciting the magnetism of P, however, a precisely contrary motion is observed—*the lever approaches the pole.* This result, which, as far as the lecturer could see, was perfectly inexplicable on the assumption that the diamagnetic force was purely repulsive, is explained in a simple and beautiful manner on the hypothesis of diamagnetic polarity. According to this, the end b of the bar of bismuth is repelled by P, and the end a is attracted: but the force acting upon a is applied at a greater distance from the axis of suspension than that acting upon b; and as it has been arranged that the absolute intensities of the forces acting upon the two ends differ very slightly from each other, the mechanical advantage possessed by a gives it the greatest moment of rotation, and the bar is attracted instead of repelled. Let a magnetic needle $n\ s$ (fig. 3) be attached like the bar $a\ b$ (fig. 3) to a lever, and submitted to the earth's magnetism. Let the north pole of the earth be towards N; the action of the pole upon n is attractive, upon s repulsive, the absolute intensities of these forces are the same, inasmuch as the length of the needle is a vanishing quantity in comparison with its distance from the pole N: hence the mechanical advantage possessed by

Fig. 2.

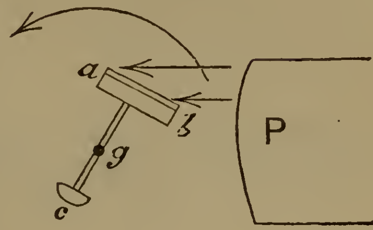


Fig. 3.

the force acting upon *s*, on account of its greater distance from the axis of rotation, causes the lever to recede from N, and we obtain a result perfectly analogous to that obtained with the bar of bismuth (fig. 2).

The metallic moulds referred to in the foregoing abstract are sketched in the annexed three figures:—

Fig. 1.

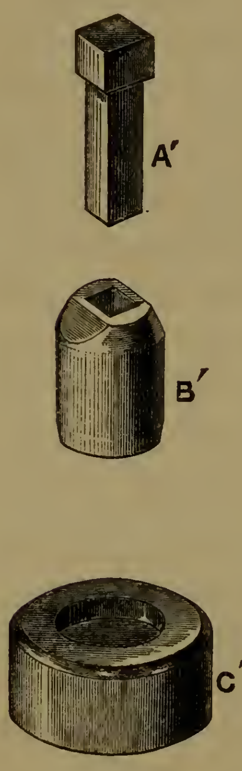


Fig. 2.

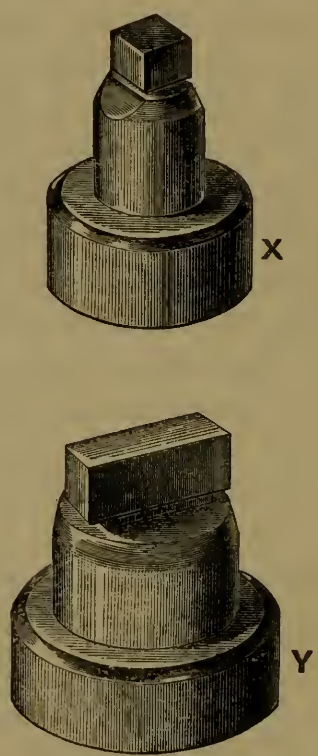
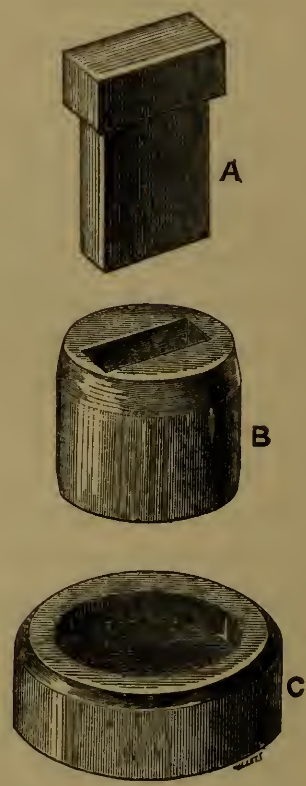


Fig. 3.



In Fig. 1, A′, B′, C′ represent the three parts of the mould used for forming cubes of compressed bismuth, whether solid and coherent, or in powder. Fig. 3, A, B, C, represent the three parts of the mould employed to form rectangular bars. In Fig. 2, X, the three parts of Fig. 1 are put together. In Fig. 2, Y, the three parts of Fig. 3 are put together. In experimenting, B′ or B is first placed upon its base, C′ or C; the solid or the powder is then placed within B′ or B, the plunger A′ or A is then introduced, and the whole squeezed between the plates of a small hydraulic press. The compressed substance is of course firmly jammed in the mould, and to remove it a *perforated* base (not shown in the figure) is employed, on which B′ or B is placed, and the squeezed metal forced out by the plunger A′ or A, acted on by the hydraulic press. The drawings are half the linear size of the moulds themselves.

XII.—DISPOSITION OF FORCE IN PARAMAGNETIC AND DIAMAGNETIC BODIES.*

The notion of an attractive force, drawing bodies towards the centre of the earth, was entertained by Anaxagoras and his pupils, by Democritus, Pythagoras, and Epicurus; and the conjectures of these ancients were renewed by Galileo, Huyghens, and others, who stated that bodies attract each other as a magnet attracts iron. Kepler applied the notion to bodies beyond the surface of the earth, and affirmed the extension of this force to the most distant stars. Thus it would appear, that in the attraction of iron by a magnet originated the conception of the force of gravitation. Nevertheless, if we look closely at the matter, it will be seen that the magnetic force possesses characters strikingly distinct from those of the force which holds the universe together. The theory of gravitation is, that every particle of matter attracts every other particle; in magnetism also we have the phenomenon of attraction, but we have also, at the same time, the fact of repulsion, and the final effect is always due to the difference of these two forces. A body may be intensely acted on by a magnet, and still no motion of translation will follow, if the repulsion be equal to the attraction. A dipping needle was exhibited: previous to magnetisation, the needle, when its centre of gravity was supported, stood accurately level; but, after magnetisation, one end of it was pulled towards the north pole of the earth. The needle, however, being suspended from the arm of a fine balance, it was shown that its *weight* was unaltered by its magnetisation. In like manner, when the needle was permitted to float upon a liquid, and thus to follow the attraction of the north magnetic pole of the earth, there was no motion of the mass towards the pole referred to; and the reason was known to be, that although the marked end of the needle was *attracted* by the north pole,

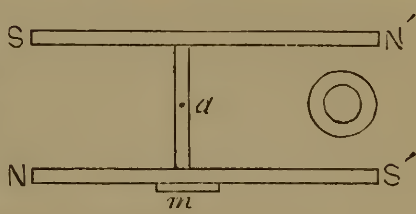
* Proceedings of the Royal Institution, vol. ii. p. 159.

the unmarked end was *repelled* equally, and these two equal and opposite forces neutralised each other as regards the production of a motion of translation. When the pole of an ordinary magnet was brought to act upon the swimming needle, the latter was attracted,—the reason being that the attracted end of the needle being much nearer to the pole of the magnet than the repelled end, the force of attraction was the more powerful of the two; but in the case of the earth, the pole being so distant, the length of the needle was practically zero. In like manner, when a piece of iron is presented to a magnet, the nearer parts are attracted, while the more distant parts are repelled; and because the attracted portions are nearer to the magnet than the repelled ones, we have a balance in favour of attraction. Here then is the most wonderful characteristic of the magnetic force, which distinguishes it from that of gravitation. The latter is a simple unpolar force, while the former is duplex or polar. Were gravitation like magnetism, a stone would no more fall to the ground than a piece of iron towards the north magnetic pole: and thus, however rich in consequences the supposition of Kepler and others may have been, it was clear that a force like that of magnetism would not be able to transact the business of the universe.

The object of the evening's discourse was to inquire whether the force of diamagnetism, which manifested itself as a repulsion of certain bodies by the poles of a magnet, was to be ranged as a polar force, beside that of magnetism; or as an unpolar force, beside that of gravitation. When a cylinder of soft iron is placed within a helix, and surrounded by an electric current, the antithesis of its two ends, or in other words, its polar excitation, is at once manifested by its action upon a magnetic needle; and it may be asked why a cylinder of bismuth may not be substituted for the cylinder of iron, and its state similarly examined. The reason is, that the excitement of the bismuth is so feeble, that it would be quite masked by that of the helix in which it is enclosed; and the problem that now meets us is, so to excite a diamagnetic body that the pure action of the body upon a magnetic needle may be observed, unmixed with the action of the body used to excite the diamagnetic.

How this may be effected, was illustrated in the following manner:—An upright helix of covered copper wire was placed

upon the table, and it was shown that the top of the helix attracted, while its bottom repelled the same pole of a magnetic needle; its central point, on the contrary, was neutral, and exhibited neither attraction nor repulsion. This helix was caused to stand between the two poles N′S′ of an astatic magnet; the two magnets S N′ and S′N were united by a rigid cross piece at their centres, and suspended from the point *a* so that both magnets swung in the same horizontal plane.



It was so arranged that the poles N′S′ were opposite to the central or neutral point of the helix, so that when a current was sent through the latter, the magnets were unaffected by the current. Here then we had an excited helix which itself had no action upon the magnets, and we were thus at liberty to examine the action of a body placed within the helix and excited by it, undisturbed by the influence of the latter. The helix was 12 inches high, and a cylinder of soft iron 6 inches long suspended from a string and passing over a pulley could be raised or lowered within the helix. When it was so far sunk that its lower end rested upon the table, the upper end found itself between the poles N′S′ attracting one of them, and repelling the other, and consequently deflecting the astatic system in a certain direction. When the cylinder was raised so that the upper end was at the level of the top of the helix, its lower end was between the poles N′S′; and a deflection opposed in direction to the former one was the immediate consequence. To render these deflections more visible, a mirror *m*, was attached to the system of magnets; a beam of light thrown upon the mirror was reflected and projected as a bright disk against the wall of the theatre; the distance of this image from the mirror being considerable, and its angular motion double that of the latter, a very slight motion of the magnet was sufficient to produce a displacement of the image through several yards. This then is the principle of the beautiful instrument* by which the investigation now brought forward was conducted.

It is manifest that if a second helix be placed between the

* Devised by Prof. W. Weber, and constructed by M. Leyser, of Leipzig.

poles SN with a cylinder within it, the action upon the astatic magnet may be exalted. This was the arrangement made use of in the actual inquiry. Thus to intensify the feeble action, which it is here our object to seek, we have in the first place neutralised the action of the earth upon the magnets, by rendering them astatic. Then, by permitting two cylinders to act simultaneously on the four poles of the magnets, we have rendered the deflecting force four times what it would be if only a single pole were used. The whole apparatus was enclosed in a suitable case, which protected the magnets from air currents, and by means of a distant telescope and scale the deflections were read off through a small glass window in the case of the instrument.

A pair of bismuth cylinders was first examined. Sending a current through the helices, and observing that the magnets swung perfectly free, it was first arranged that the cylinders within the helices had their central points opposite to the poles of the magnets. All being at rest the number on the scale marked by the cross wire of the telescope was 572. The cylinders were then moved so that the two ends were brought to bear simultaneously upon the magnetic poles: the magnet moved promptly, and after some oscillations * came to rest at the number of 612; thus moving from a smaller to a larger number. The other two ends of the bars were next brought to bear upon the magnet: a prompt deflection was the consequence, and the final position of equilibrium was 526; the movement being from a larger to a small number. We thus observe a manifest polar action of the bismuth cylinders upon the magnet; one pair of ends deflecting it in one direction, and the other pair deflecting it in the opposite direction.

Substituting for the cylinders of bismuth thin cylinders of iron, of magnetic slate, of sulphate of iron, carbonate of iron, protochloride of iron, red ferrocyanide of potassium, and other magnetic bodies, it was found that when the position of the magnetic cylinders was the same as that of the cylinders of bismuth, the deflection produced by the former was always opposed in direction to that produced by the latter; and hence the disposition of the force in the diamagnetic body must have been precisely antithetical to its disposition in the magnetic ones.

* To lessen these a copper damper was made use of.

But it will be urged, and indeed has been urged against this inference, that the deflection produced by the bismuth cylinders is purely due to the currents of induction excited in the mass by its motion within the helices. In reply to this objection, it may be stated, in the first place, that the deflection is permanent, and cannot therefore be due to induced currents, which are only of momentary duration. It has also been urged that such experiments ought to be made with other metals, and with better conductors than bismuth, for if due to currents of induction the better the conductor the more exalted will be the effect. This requirement was complied with.

Cylinders of antimony were substituted for those of bismuth. This metal is a better conductor of electricity, but less strongly diamagnetic than bismuth. If therefore the action referred to be due to induced currents, we ought to have it greater in the case of antimony than with bismuth; but if it springs from a true diamagnetic polarity, the action of the bismuth ought to exceed that of the antimony. Experiment proves that the latter is the case, and that hence the deflection produced by these metals is due to their diamagnetic, and not to their conductive capacity. Copper cylinders were next examined; here we have a metal which conducts electricity fifty times better than bismuth, but its diamagnetic power is nearly null; if the effects be due to induction we ought to have them here in an enormously exaggerated degree, but no sensible deflection was produced by the two cylinders of copper.

It has also been proposed by the opponents of diamagnetic polarity to coat fragments of bismuth with some insulating substance, so as to render the formation of induced currents impossible, and to test the question with cylinders of these fragments. This requirement was also fulfilled. It is only necessary to reduce the bismuth to powder and expose it for a short time to the air to cause the particles to become so far oxidised as to render them perfectly insulating. The power of the powder in this respect was exhibited experimentally in the lecture; nevertheless, this powder, enclosed in glass tubes, exhibited an action scarcely less powerful than that of the massive cylinders.

But the most rigid proof, a proof admitted to be conclusive by those who have denied the antithesis of magnetism and diamagnetism, remains to be stated. Prisms of the same heavy

glass as that with which the diamagnetic force was discovered, were substituted for the metallic cylinders, and their action upon the magnet was proved to be precisely the same in kind as that of the cylinders of bismuth. The inquiry was also extended to other insulators: to phosphorus, sulphur, nitre, calcareous spar, statuary marble, with the same invariable result: each of these substances was proved polar, the disposition of the force being the same as that of bismuth and the reverse of that of iron. When a bar of iron is set erect, its lower end is known to be a north pole, and its upper end a south pole, in virtue of the earth's induction. A marble statue, on the contrary, has its feet a south pole, and its head a north pole, and there is no doubt that the same remark applies to its living archetype; each man walking over the earth's surface is a true diamagnet, with its poles the reverse of those of a mass of magnetic matter of the same shape and in a similar position.

An experiment of practical value, as affording a ready estimate of the different conductive powers of two metals for electricity, was exhibited, for the purpose of proving experimentally some of the statements made in reference to this subject during the discourse. A cube of bismuth was suspended by a twisted string between the two poles of an electro-magnet. The cube was attached by a small copper wire to a little square pyramid, the base of which was horizontal, its sides being formed of four small triangular pieces of looking-glass. A beam of light was suffered to fall upon this reflector, and as it followed the motion of the cube a succession of images was cast from its sides, each describing a circle of about 30 feet in diameter on the walls of the room. Through the persistence of the impression upon the retina these images finally blended into a continuous ring of light. At a particular instant the electro-magnet was excited, currents were evolved in the rotating cube, and the strength of these currents was practically estimated by the time required by the magnet to bring the cube and its associated mirrors to rest. With bismuth this time amounted to a score of seconds or more: a cube of copper, on the contrary, was struck almost instantly motionless when the circuit was established.*

* See 'Heat as a Mode of Motion,' par. 31.

XIII.—CURRENTS OF THE LEYDEN BATTERY.*

In our conceptions and reasonings regarding the forces of nature we perpetually make use of symbols, which, when they possess a high representative value, we dignify with the name of theories. We observe, for example, heat propagating itself through a bar of metal, and help ourselves to a conception of the process by comparing it with water percolating through sand, or travelling by capillary attraction through a lump of sugar. In some such way we arrive at what is called the material theory of heat. The thing seen is thus applied to the interpretation of the thing unseen, and the longing of the human mind to rest upon a satisfactory reason, is in some measure satisfied. So also as regards the subject of the present evening's discourse; we are not content with the mere facts of electricity; we wish to look behind the facts, and prompted by certain analogies we ascribe electrical phenomena to the action of a peculiar fluid. Such conceptions have their advantages and their disadvantages: they afford peaceful lodging to the intellect for a time, but they also circumscribe it; and by and by, when the mind has grown too large for its mansion, it often finds a difficulty in breaking down the walls of what has become its prison instead of its home. Thus at the present day, the man who would cross the bounds which at present limit our knowledge of electricity and magnetism finds it a work of extreme difficulty to look at facts in their simplicity, or to rid them of those hypothetical adornments with which common consent has long invested them.

But though such is the experience of the earnest student of Natural Philosophy at the present time—though he may be compelled to refuse his assent to the prevalent theoretic notions, he may nevertheless advantageously make use of the language of these theories in bringing the facts of a science before a public

* Proceedings of the Royal Institution, vol. ii. p. 132.

audience; and in speaking of electricity, the speaker availed himself of the convenient hypothesis of two fluids, without at all professing a belief in their existence. A Leyden jar was charged. The interior of the jar might be figured as covered with a layer of positive electricity, and the exterior by a layer of negative electricity; which two electricities, notwithstanding their mutual attraction, were prevented from rushing together by the glass between them. When the exterior and interior coating are united by a conducting body, the fluids move through the conductor and unite; thus producing what is called an electric current. The mysterious agent which we darkly recognise under this symbol is capable of producing wonderful effects; but one of its most wonderful characteristics is its power of arousing a transitory current in a conductor placed near it. The phenomena of voltaic induction are well known; and it is interesting to enquire whether frictional electricity produces analogous phenomena. This question has been ably examined by Dr. Henry, and still more recently by that skilful and profound electrician M. Riess, of Berlin. The researches of these gentlemen constituted the subject of the evening's discourse.

Two copper wires, each 75 feet in length, were wound round a wooden cylinder. Both wires were placed upon a surface of gutta-percha, and kept perfectly insulated from each other. The ends of one of them were connected with a universal discharger, the knobs of which were placed within a quarter of an inch of each other. When the current of a Leyden battery was sent through the other wire, a secondary current was aroused in that connected with the discharger, which announced itself by a brilliant spark passing across the space separating the two knobs.

The wires here used were covered externally with a sheet of gutta-percha. It might, perhaps, be supposed that a portion of the electricity of the battery had sprung from the one wire to the other; two flat disks were, therefore, next employed. Each disk contained 75 feet of copper wire, wound in the form of a flat spiral, the successive convolutions of which were about two lines apart. One disk was placed upon the other, the wire being so coiled that the convolutions of each disk constituted, so to say, the imprint of those of the other,

and the coils were separated from each other by a plate of varnished glass. The ends of one spiral were connected with the universal discharger, between the knobs of which a thin platinum wire, ten inches long, was stretched. When the current of the Leyden battery was sent through the other spiral, a secondary current passed through the thin wire, and burnt it up with brilliant deflagration. A pair of spirals were next placed six inches apart, and a battery was discharged through one of them; the current aroused in the other was sufficient to deflagrate a thin platinum wire four inches in length.

We have every reason to suppose that the secondary current thus developed is of the same nature as the primary which produced it; and hence we may infer, that if we conduct the secondary away and carry it through a second spiral, it, in its turn, will act the part of a primary, and evoke a *tertiary* current in an adjacent spiral. This was illustrated by experiment. First, two spirals were placed opposite to each other, through one of which the current of the battery was to be sent; the other was that in which the secondary current was to be aroused. The ends of the latter were connected by wires with a third spiral placed at a distance, so that when the secondary current was excited it passes through the third spiral. Underneath the latter, and separated from it by a sheet of varnished glass, was a fourth spiral, whose two ends were connected with the universal discharger, between the knobs of which a quantity of gun-cotton was placed. When the battery was discharged through the first spiral, a secondary current was aroused in the second spiral, which completed its circuit by passing through the third spiral: here the secondary acted upon the spiral underneath, developed a tertiary current which was sufficiently strong to pass between the knobs, and to ignite the gun-cotton in its passage. It was shown that we might proceed in this way and cause the tertiary to excite a current of the fourth order, the latter a current of the fifth order, and so on; these children, grandchildren, and great grandchildren of the primary being capable of producing all the effects of their wonderful progenitor.

The phenomena of the *extra current*, which exists for an instant contemporaneously with the ordinary current in a common voltaic helix, were next exhibited; and the question whether a spiral through which a Leyden battery was discharged

exhibited any similar phenomena was submitted to examination. It was proved, that the electric discharge depended upon the *shape* of the circuit through which it passed: when two portions of such a circuit are brought near each other, so that the positive electricity passes in the same direction through both of them, the discharge is *weaker* than if sent through a straight wire: if, on the contrary, the current flow through both portions in opposite directions, the discharge is *stronger* than if it had passed through a straight wire. One end of a flat spiral containing 75 feet of copper wire was connected with a knob of the universal discharger, and the other knob was connected with the earth; between the knobs of the discharger about four inches of platinum wire were stretched. On connecting the other end of the spiral with the battery a discharge passed through it which was quite unable to raise the platinum wire to the faintest glow. The same length of copper wire was then bent to and fro in a zigzag manner, so that on every two adjacent legs of the zigzag the current from the battery flowed in opposite directions. When the 75 feet of wire were interposed between the battery and the platinum wire, a discharge precisely equal to that used in the former instance, raised the platinum wire to a high state of incandescence, and indeed could be made to destroy it altogether.

When a primary and a secondary spiral are placed opposite to each other, a peculiar reaction of the secondary upon the primary is observed. If the ends of a secondary (50 feet long) be connected by a thick wire, the effect upon the primary current is the same as when the ends of the secondary remain wholly unconnected. If the ends of the secondary be joined by a long thin platinum wire, the reaction of the secondary is such as to enfeeble the primary. This enfeeblement increases up to a certain limit as the resistance is increased, from which forwards it diminishes until it becomes insensible. This would appear to prove that to react upon the primary the secondary requires to be retarded; and that the greater the amount of the retardation, up to a certain limit, the greater is the enfeeblement. But by increasing the resistance we diminish the strength of the secondary, and when a certain limit is attained, this diminution is first compensated by the influence of retardation, from which point forwards with every increase of

the resistance, the enfeeblement of the primary is diminished. A primary current which fuses a certain length of platinum wire where the ends of the secondary are disunited, or where they are united by a thick wire, fails to do so when they are united with a thin wire. But if, instead of a thin wire, a body of much greater resistance, a column of water for example, be introduced, the platinum wire is fused as before.

XIV.—INFLUENCE OF MAGNETIC FORCE ON THE ELECTRIC DISCHARGE.*

In this discourse a series of experiments illustrative of the constitution of the electric discharge, and of the action of magnetism upon it, were brought before the members of the Royal Institution. The matter of the discourse was derived from the researches of various philosophers, its form being regulated to suit the requirements of the audience.

1. The influence of the transport of particles was shown by an experiment first suggested, it was believed, by Sir John Herschel, and performed by Professor Daniell. The carbon terminals of a battery of 40 cells of Grove were brought within one-eighth of an inch of each other, and the spark from a Leyden jar was sent across this space. This spark bridged with carbon particles the gap which had previously existed in the circuit, and the brilliant electric light due to the passage of the battery current was immediately displayed.

2. The magnified image of the carbon points of an electric lamp was projected upon a white screen, and the distance to which they could be drawn apart without interrupting the current was noted. A button of pure silver was then introduced in place of the positive carbon, a luminous discharge four or five times the length of the former being thus obtained. The silver was first observed to glow, and afterwards to pass into a state of violent ebullition. A narrow dark space was observed to surround one of the poles, corresponding probably with the dark space observed in the discharge of Ruhmkorff's coil through rarefied media.†

3. The action of a magnet upon the splendid stream of green light obtained in the foregoing experiment was exhibited. A

* Proceedings of the Royal Institution, vol. iii. p. 169.

† Mr. Faraday noticed this dark stripe while I was making my preparatory experiments.—J. T.

small horseshoe magnet of Logeman was caused to approach the light, which was bent hither and thither, according as the poles of the magnet changed their position: the discharge in some cases formed a magnificent green bow, which on the further approach of the magnet was torn asunder, and the passage of the current thereby interrupted. It was Davy who first showed the action of a magnet upon the voltaic arc. The transport of matter by the current was further illustrated by a series of deposits on glass obtained by Mr. Gassiot from the continued discharge of an induction coil.

4. A discharge from Ruhmkorff's coil was sent through an attenuated medium; and the glow, which surrounded the negative electrode, was referred to. One of the most remarkable effects hitherto observed was that of a magnet upon this negative light. Plücker had shown that it arranges itself under the influence of the magnet exactly in the direction of the magnetic curves. Iron filings strewn in space, and withdrawn from the action of gravity, would arrange themselves around a magnet exactly in the manner of the negative glow.

An electric lamp was placed upon its back; a horseshoe magnet was placed horizontally over its lens, and on the magnet a plate of glass: a mirror inclined at an angle of 45° received the beam from the lamp, and projected it upon the screen. Iron filings were scattered on the glass, and the magnetic curves thus illuminated were magnified, and brought to clear definition upon the screen. The negative light above referred to arranges itself, according to Plücker, in a similar manner.

5. The rotation of an electric current round the pole of a magnet, discovered by Mr. Faraday nearly forty years ago, was next shown; and the rotation of a luminous current from an induction coil in an exhausted receiver by the same magnet was also exhibited, and both shown to obey the same laws. This beautiful experiment was devised by De la Rive.

6. Into a circuit of 20 cells a large coil of copper wire was introduced, and when the current was interrupted, a bright spark, due to the passage of the extra current, was obtained. The brightness and loudness of the spark were augmented when a core of soft iron was placed within the coil. The disruption of

the current took place between the poles of an electro-magnet; and when the latter was excited, an extraordinary augmentation of the loudness of the spark was noticed. This effect was first obtained by Page, and was for a time thought to denote a new property of the electric current.

But Rijke had shown in a paper, the interest of which is by no means lessened by the modesty with which it is written, that the effect observed by Page is due to the sudden extinction of the primary spark by the magnet; which suddenness concentrates the entire force of the extra current into a moment of time. Speaking figuratively, it was the concentration of what, under ordinary circumstances, is a mere push, into a sudden shock of projectile energy.

7. The contact-breaker of an induction coil was removed, and a current from five cells was sent through the primary wire. The terminals of the secondary wire being brought very close to each other, when the primary was broken by the hand, a minute spark passed between the terminals of the secondary. When the disruption of the primary was effected between the poles of an excited electro-magnet, the small spark was greatly augmented in brilliancy. The terminals were next drawn nearly an inch apart. When the primary was broken between the excited magnetic poles, the spark from the secondary jumped across this interval, whereas it was incompetent to cross one-fourth of the space when the magnet was not excited. This result was also obtained by Rijke; who rightly showed, that in this case also the augmented energy of the secondary current was due to the augmented speed of extinction of the primary spark between the excited poles. This experiment illustrated in a most forcible manner the important influence which the mode of breaking contact may have upon the efficacy of an induction coil.

The splendid effects obtained from the discharge of Ruhmkorff's coil through exhausted tubes were next referred to. The presence of the coil had complicated the theoretic views of philosophers with regard to the origin of those effects; the intermittent action of the contact-breaker, the primary and secondary currents, and their mutual reactions, producing tertiary and other currents of a higher order, had been more or less invoked by theorists, to account for the effects observed.

Mr. Gassiot was the first to urge, with a water battery of 3,500 cells, a voltaic spark across a space of air, *before* bringing the electrodes into contact; with the self-same battery he had obtained discharges through exhausted tubes, which exhibited all the phenomena hitherto observed with the induction coil. He thus swept away a host of unnecessary complications which had entered into the speculations of theorists upon this subject.

8. On the present occasion, through the kindness of Mr. Gassiot, the speaker was enabled to illustrate the subject by means of a battery of 400 of Grove's cells. The tension at the ends of the battery was first shown by an ordinary gold-leaf electroscope; one end of the battery being insulated, a wire from the other end was connected with the electroscope; the leaves diverged; on now connecting the insulated end with the earth, the electroscopic tension rose, according to a well-known law, and the divergence was greatly augmented.

9. A large receiver (selected from Mr. Gassiot's fine collection), in which a vacuum had been obtained by filling it with carbonic acid gas, exhausting it, and permitting the residue to be absorbed by caustic potash, was placed equatorially between the poles of the large electro-magnet. The jar or receiver was about six inches wide, and the distance between its electrodes was ten inches. The negative electrode consisted of a copper dish, four inches in diameter; the positive one was a brass wire.

On the 16th of this month an accident occurred to this jar. Mr. Faraday, Mr. Gassiot, and the speaker had been observing the discharge of the nitric acid battery through it. Stratified discharges passed when the ends of the battery were connected with the electrodes of the receiver; and on one occasion the discharge exhibited an extraordinary effulgence; the positive wire emitted light of dazzling brightness, and finally gave evidence of fusion. On interrupting the circuit, the positive wire was found to be shortened about half an inch, its metal having been scattered by the discharge over the interior surface of the jar.

10. The receiver in this condition was placed before the audience in the position mentioned above. When the ends of the 400-cell battery were connected with the wires of the receiver, *no discharge passed*; but on touching momentarily with

the finger any portion of the wire between the positive electrode of the receiver and the positive pole of the battery, a brilliant discharge instantly passed, and continued as long as the connexion with the battery was maintained. This experiment was several times repeated: the connexion with the ends of the battery was not sufficient to produce the discharge, but in all cases the touching of the positive wire caused the discharge to flash through the receiver.

Previous to the fusion of the wire above referred to, this discharge usually exhibited fine stratifications: its general character now was that of a steady glow, through which, however, intermittent luminous gushes took place, each of which presented the stratified appearance.

11. On exciting the magnet between the poles of which the receiver was placed, the steady glow curved up or down according to the polarity of the magnet, and resolved itself into a series of effulgent transverse bars of light. These appeared to travel from the positive wire along the surface of the jar. The deflected luminous current was finally extinguished by the action of the magnet.

12. When the circuit of the magnet was made and immediately interrupted, the appearance of the discharge was extremely singular. At first the strata rushed from the positive electrode along the upper surface of the jar, then stopped, and appeared to return upon their former track, and pass successively with a deliberate motion into the positive electrode. They were perfectly detached from each other; and their successive engulfments at the positive electrode were so slow as to be capable of being counted aloud with the greatest ease. This deliberate retreat of the strata towards the positive pole was due, no doubt, to the gradual subsidence of the power of the magnet. Artificial means might probably be devised to render the recession of the discharge still slower. The rise of power in the magnet was also beautifully indicated by the deportment of the current.

After the current had been once quenched, as long as the magnet remained excited, no discharge passed: but on breaking the magnet circuit, the luminous glow reappeared. Not only then is there an action of the magnet upon the particles transported by an electric current, but the above experiment

indicates that there is an action of the magnet *upon the electrodes themselves, which actually prevents the escape of their particles.* The influence of the magnet upon the electrode would thus appear to be *prior* to the passage of the current.

13. The discharge of the battery was finally sent through a tube, whose platinum wires were terminated by two small balls of carbon: a glow was first produced; but on heating a portion of the tube containing a stick of caustic potash, the positive ball sent out a luminous protrusion, which subsequently detached itself from the ball; the tube becoming instantly afterwards filled with the most brilliant strata. There can be no doubt that the superior effulgence of the bands obtained with this tube is due to the character of its electrodes: *the bands are the transported matter of these electrodes.* Is not this the case with the other electrodes? There appears to be no uniform flow in nature; we cannot get either air or water through an orifice in a uniform stream; the friction against the orifice is overcome by starts, and the jet issues in pulsations. Let a lighted candle be quickly passed through the air; the flame will break itself into a beaded line in virtue of a similar intermittent action, and it may be made to sing, so regular are the pulses produced by its passage. Analogy might lead us to suppose that the electricity overcomes the resistance at the surface of its electrode in a similar manner, escaping from it in tremors; the matter which it carries along with it being broken up into strata, as a liquid vein is broken into drops.*

* Mr. Gassiot has shown that a *single discharge* of the Leyden jar produces the stratification. May not every such discharge correspond to a single draw of a violin bow across a string?

XV.—THE MAGNETIC FIELD AND THE ELECTRIC CURRENT.

The following extract from a chapter of 'Faraday as a Discoverer' indicates the notions more or less vague which have been for a good while passing through my mind with regard to the character of the magnetic field and the nature of an electric current. For the most important contribution to this subject hitherto made I must refer to Mr. Clerk Maxwell's 'Dynamical Theory of the Electro-magnetic Field,' Phil. Trans. 1865, p. 459.

'These considerations will help to clear our way to the conception of the transformations which occur when a wire is moved across the lines of force in a magnetic field. In this case it is commonly said we have a conversion of magnetism into electricity. But let us endeavour to understand what really occurs. For the sake of simplicity, and with a view to its translation into a different one subsequently, let us adopt for a moment the provisional conception of a mixed fluid in the wire, composed of positive and negative electricities in equal quantities, and therefore perfectly neutralising each other when the wire is still. By the motion of the wire, say with the hand, towards the magnet, what the Germans call a *Scheidungs-Kraft* —a separating force—is brought into play. This force tears the mixed fluids asunder, and drives them in two currents, the one positive, and the other negative, in two opposite directions through the wire. The presence of these currents evokes a force of *repulsion* between the magnet and the wire; and to cause the one to approach the other, this repulsion must be overcome. The overcoming of this repulsion is, in fact, the work done in separating and impelling the two electricities. When the wire is moved away from the magnet, a *Scheidungs-Kraft*, or separating force, also comes into play; but now it is

an *attraction* that has to be surmounted. In surmounting it, currents are developed in direction opposed to the former; positive takes the place of negative, and negative the place of positive; the overcoming of the attraction being the work done in separating and impelling the two electricities.

'The mechanical action occurring here is different from that occurring where a sphere of soft iron is withdrawn from a magnet, and again attracted. In this case muscular force is expended during the act of separation; but the attraction of the magnet effects the reunion. In the case of the moving wire, also, a resistance is overcome in separating the wire from the magnet, and thus far the action is mechanically the same as the separation of the sphere of iron. But after the wire has ceased moving, the attraction ceases; and so far from any action occurring similar to that which draws the iron sphere back to the magnet, we have to overcome a repulsion to bring them together.

'There is no potential energy conferred either by the removal or by the approach of the wire, and the only power really transformed or converted, in the experiment, is muscular power. Nothing that could in strictness be called a conversion of magnetism into electricity occurs. The muscular oxidation that moves the wire fails to produce *within the muscle* its due amount of heat, a portion of that heat equivalent to the resistance overcome, appearing in the moving wire instead.

'Is this effect an attraction and a repulsion at a distance? If so, why should both cease when the wire ceases to move? The deportment of the wire resembles far more that of a body moving *in a resisting medium* than anything else; the resistance ceasing when the motion is suspended. Let us imagine the case of a liquid so mobile that the hand may be passed through it to and fro, without encountering any sensible resistance. It resembles the motion of a conductor in the unexcited field of an electro-magnet. Now let us suppose a body placed in the liquid, or acting on it, which confers upon it the property of *viscosity*; the hand would no longer move freely. During its motion, but then only, resistance would be encountered and overcome. Here we have rudely represented the case of the excited magnetic field, and the result in both cases would be substantially the same. In both cases heat

would, in the end, be generated outside of the muscle, its amount being exactly equivalent to the resistance overcome.

'Let us push the analogy a little further. Suppose in the case of the fluid rendered viscous, as assumed a moment ago, the viscosity not to be so great as to prevent the formation of *ripples* when the hand is passed through the liquid. Then the motion of the hand, before its final conversion into heat, would exist for a time as more rapid wave-motion, which on subsiding would generate its due equivalent of heat. This intermediate stage, in the case of our moving wire, is represented by the period *during which the electric current is flowing through it*; but that current, like the ripples of our liquid, soon subsides, being, like them, converted into heat.

'Do these words shadow forth anything like the reality? Such speculations cannot be injurious if they are enunciated without dogmatism. I do confess that ideas such as these here indicated exercise a strong fascination on my mind. Is then the magnetic field really viscous, and if so, what substance exists in it and the wire to produce the viscosity? Let us first look at the proved effects, and afterwards turn our thoughts back upon their cause. When the wire approaches the magnet, an action is evoked within it, which travels through it with a velocity comparable to that of light. One substance only in the universe can be referred to as competent to transmit power with this velocity,—the luminiferous ether. Not only its rapidity of progression but its ability to produce the motion of light and heat, indicates that the electric current is also motion.* Further, there is a striking resemblance between the action of good and bad conductors as regards electricity, and the action of diathermanous and adiathermanous bodies as regards radiant heat. The good conductor is diathermanous to the electric current; it allows free transmission without the development of heat. The bad conductor is adiathermanous to the electric current, and hence the passage of the latter is accompanied by the development of heat. I am

* Mr. Clerk Maxwell has recently published an exceedingly important investigation connected with this question. Even in the non-mathematical portions of the memoirs of Mr. Maxwell, the admirable spirit of his philosophy is sufficiently revealed. As regards the employment of scientific imagery, I hardly know his equal in power of conception and clearness of definition.

strongly inclined to hold the electric current, pure and simple, to be a motion of the ether alone; good conductors being so constituted that the motion may be propagated through their ether without sensible transfer to their atoms, while in the case of bad conductors this transfer is effected, the transferred motion appearing as heat.' *

* One important difference, of course, exists between the effect of motion in the magnetic field, and motion in a resisting medium. In the former case the heat is generated *in the moving conductor*, in the latter it is in part generated *in the medium.*

XVI.—ON THE REDUCTION OF TEMPERATURES BY ELECTRICITY.*

To the Editors of the 'Philosophical Magazine and Journal.'

Gentlemen,—In an abstract of Professor William Thomson's 'Mechanical Theory of Thermo-electric Currents,' given in your Supplementary Number for July, reference is made to the well-known experiment of Peltier on the absorption of heat at a bismuth and antimony joint. This has induced Mr. Adie to publish a brief communication in your Number for September, from which it appears that the writer has never been able to obtain Peltier's result. He virtually denies its existence, and affirms the true state of the case to be that *less* heat is developed at some junctions than at others, but that *cold* is never generated. An objection precisely similar to this induced Lenz to repeat Peltier's experiment fifteen years ago;† and to the experiment of Lenz I took the liberty of directing Mr. Adie's attention in your October Number. I did so because Mr. Adie had not mentioned it in his remarks, and it seemed to me to offer a proof of the absorption of heat so obvious as to be immediately appreciated. It does not however appear so to Mr. Adie, for in your last Number he suggests a hygrometric action as the probable cause of the diminution of temperature observed by Lenz. I do not now propose to occupy your space in dwelling upon conjectures, but to resort at once to the 'law and testimony' of experiment.

Experiment No. 1.—In plate V. fig. 1, A is a bar of antimony, B a bar of bismuth, both bars being brought into close contact at J. To the free ends of the bars the wires *w w′* are soldered, and dip into the little pools of mercury *m m′*; *c* is a piece of cork through which the wires pass, and by which the wires *w w′* may be easily moved from the pools *m m′* to

* Phil. Mag. vol. iv. p. 419.

† Poggendorff's *Annalen*, vol. xliv. p. 342.

Fig. 1.

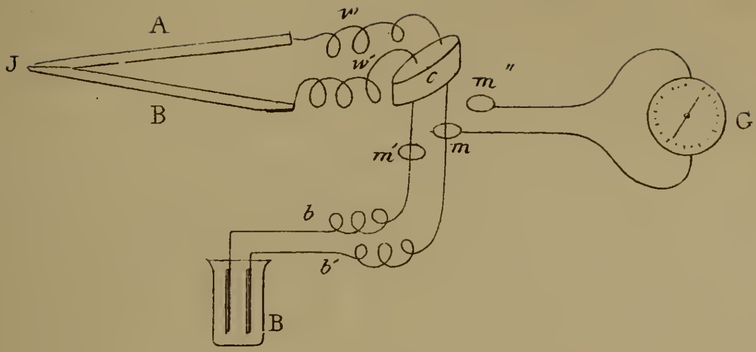


Fig. 2.

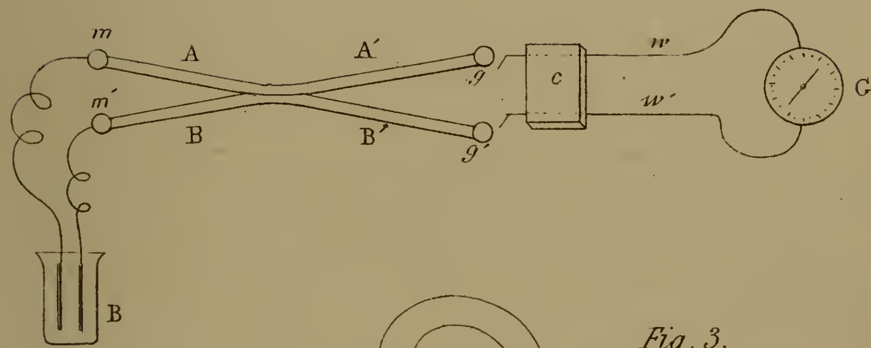


Fig. 3.

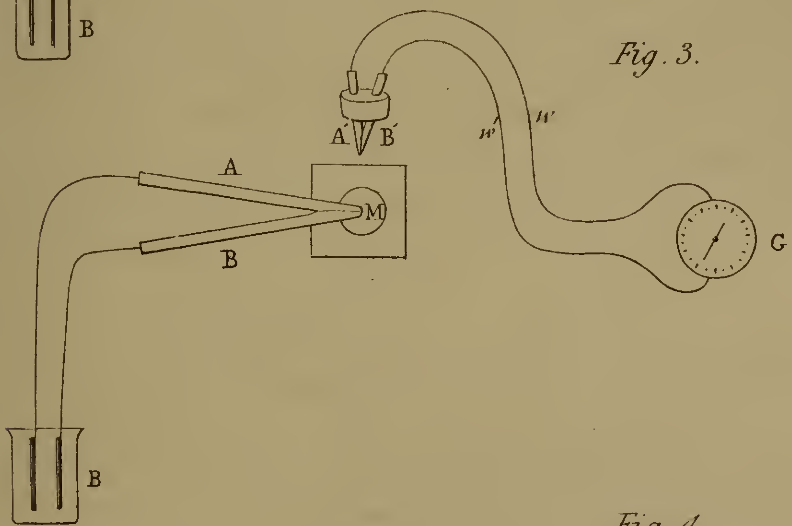
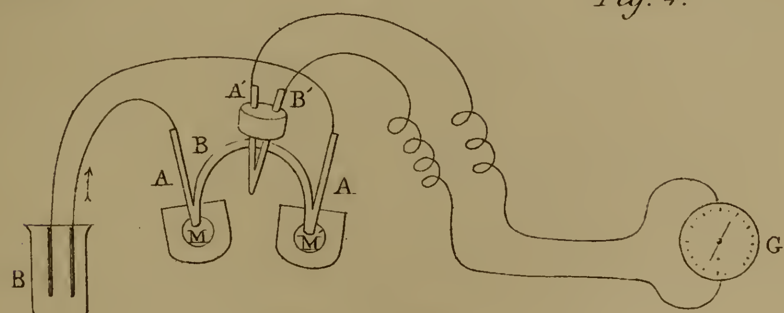


Fig. 4.



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$m\,m''$, the warming of the wires being prevented by the cork. From $m\,m''$ wires proceed to a galvanometer, G, whose needles prove themselves to be perfectly astatic by setting at right angles to the magnetic meridian.* B is a single cell of Bunsen, from which, when matters stand as in the figure, a current can be sent through the bismuth and antimony pair.

The voltaic circuit having been established, the current—a very feeble one—was permitted to circulate for two minutes, its direction being from antimony to bismuth across the junction; at the end of the time specified the wires $w\,w'$ were moved from $m\,m'$ to $m\,m''$, a thermo-electric circuit being thus formed in which the galvanometer was included; the index of the instrument was at once deflected, and the extreme limit of its first impulsion was noted; it amounted to

75°.

The deflection in this case was similar in direction to that produced when the warm finger was placed upon the junction.

The wires $w\,w'$ were moved back to their former position, and the apparatus was suffered to cool; by crossing the wires $b\,b'$, causing the former to dip into m and the latter into m', the voltaic current was reversed, its direction across the junction being now from bismuth to antimony; the same time of circulation being allowed, on establishing the thermo-electric circuit, as before, a deflection of

68°

was observed. The deflection was the same as that produced when a small glass containing a *freezing mixture* was placed upon the junction.

But Mr. Adie will probably urge, that it is not the cold developed at J, but the heat developed at some of the other points in the circuit, which caused the deflection here. I will not pause to discuss the objection, but will proceed to an experiment which deprives it of all force.

Experiment No. 2.—AA′ is a bar of antimony, BB′ is a bar of bismuth cast as in fig. 2, and in contact at the centre. From the cell B a current was sent through the system, and during its circulation the ends $g\,g'$ were unconnected; neither heating nor cooling of these ends by the current was therefore

* See 'Heat as a Mode of Motion.' Appendix to Lecture I.

possible. The direction of the current across the junction was first from antimony to bismuth. After a short period of circulation the current was interrupted, and the ends of the wires *w w′* were dipped into the mercury cups *g g′*, which were also in contact with A′B′; the index was driven through an arc of

40°.

The deflection in this case showed that the junction had been *heated.*

The current was reversed, its direction across the junction being now from bismuth to antimony; proceeding as before, the deflection was

30°.

This deflection was the same as that produced when the temperature of the junction was *lowered* by a freezing mixture.

I see no escape here from the conclusion that heat has been absorbed. For the ends *g g′*, exposed as they are to the atmosphere, must have its temperature, while the ends *m m′*, on which suspicion might reasonably rest, the current having passed through them, are wholly excluded from the thermo-electric circuit. The reader will observe that this is merely a modification of Lenz's experiment with the metallic cross.

But Mr. Adie has tried the cross, and it does not satisfy him. Let us then discard it, and proceed at once to an *experimentum crucis.* If the arms A′ B′ are not actually included in the voltaic circuit, they may seem to be in suspicious connection with it. We must remove this source of doubt.

Experiment No. 3.—A and B, fig. 3, represent, as before, the bismuth and antimony couple, united at one end. M is a small chamber, hollowed out in a piece of cork and filled with mercury. A′ B′ is a second delicate thermo-electric pair, connected with the galvanometer, but wholly unconnected with AB. The wires *w w′* are sufficiently strong to support A′ B′, so that the junction stands vertically over M, a slight pressure being sufficient to cause the wedge-shaped end of the pair to descend into the chamber of mercury. The whole arrangement was permitted to remain in a room until the temperature of the surrounding atmosphere was attained. Matters being in this state, when the pair A′ B′, which I will call the *test-pair,* was dipped into the mercury M, no effect was produced on the galvanometer.

Now the mercury must partake of the changes of temperature of the junction with which it is in contact, and the nature of these changes will be ascertained with great precision by examining the mercury at proper intervals by means of the test-pair.

The voltaic circuit was closed, and the current allowed to circulate for three minutes, passing in the first place from bismuth to antimony. The current was then interrupted, and the test-pair was immediately dipped into the pool of mercury; the index of the galvanometer was driven through an arc of

40°.

The deflection was similar to that produced by immersing the end of the test-pair in a freezing mixture. Hence in this case heat was undoubtedly *abstracted from the mercury* during the passage of the current.

The apparatus being permitted to resume its equilibrium, the voltaic current was caused to traverse AB in an opposite direction. At the end of three minutes the test-pair was again immersed, and a deflection of

45°

was the consequence. The deflection was opposed to the former one, and demonstrated the *generation of heat* at the junction.

I am at present unable to see what possible objection can be brought against this last experiment. A hygrometric effect is out of the question; and the test-pair A′ B′, being wholly unconnected with the voltaic current, cannot in any way be influenced by the latter. The results observed are evidently pure effects of the heating and cooling of the junction.

The following experiment exhibits all the necessary evidence without the reversion of the voltaic current.

Experiment No. 4.—B, fig. 4, is a curved bar of bismuth, with each end of which a bar of antimony, A, is brought into close contact. In front of the two junctions are chambers, hollowed out in cork and filled with mercury as before. A current was sent from the cell B in the direction indicated by the arrow; at M it passed from antimony to bismuth, and at M′ from bismuth to antimony. Now if Peltier's observation be correct, we ought to have the mercury at M warmed, and that at M′ cooled

by the passage of the current. After three minutes' circulation the voltaic circuit was broken, and the test-pair dipped into M′; the consequent deflection was

$$38°,$$

and the sense of the deflection proved that at M′ heat had been *absorbed.*

The needles were brought quickly to rest at zero, and the test-pair was dipped into M; the consequent deflection was

$$60°;$$

the sense of the deflection proved that at M heat had been *generated.*

The systems of bars represented in fig. 4, being imbedded in wood, the junction at M cooled slowly, and would have taken a quarter of an hour at least to assume the temperature of the atmosphere. The voltaic current was reversed, and three minutes' action not only absorbed all the heat at M, but generated cold sufficient to drive the needle through an arc of 20° on the negative side of zero.

These experiments, gentlemen, corroborate those of Peltier. Nevertheless I would say, that the conclusions of Mr. Adie are such as a restricted examination of the subject will most probably lead to. I have no doubt as to the correctness of his results described in the September Number of the Magazine; but I have just as little doubt that, had Mr. Adie sufficiently varied the strength of his current, he would have found reason to modify the statement, that 'in his experiments he had never met a fact which in the least encourages the view that electricity reduces temperatures.'

I remain, Gentlemen,
Your obedient servant,
JOHN TYNDALL.

Fig. 1.

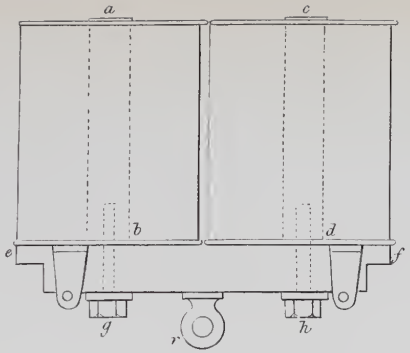


Fig. 2.

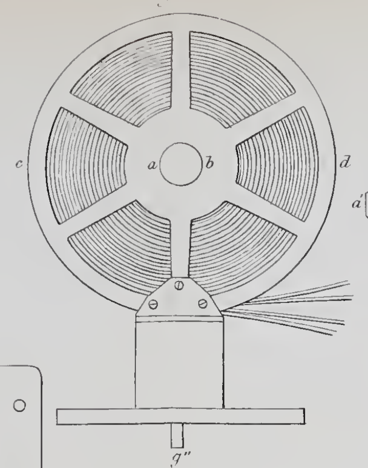


Fig. 7.

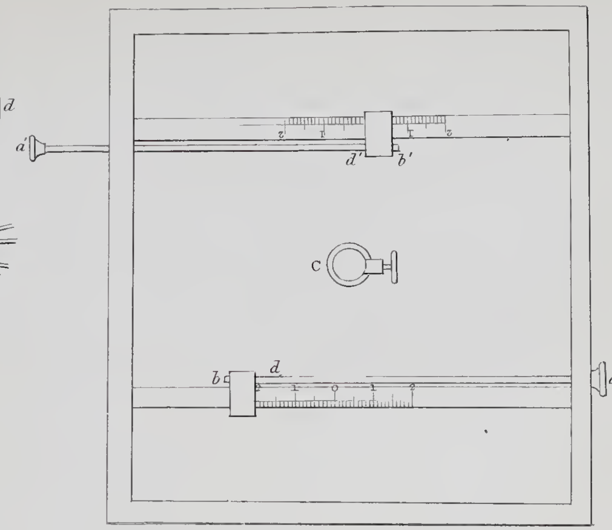


Fig. 8.

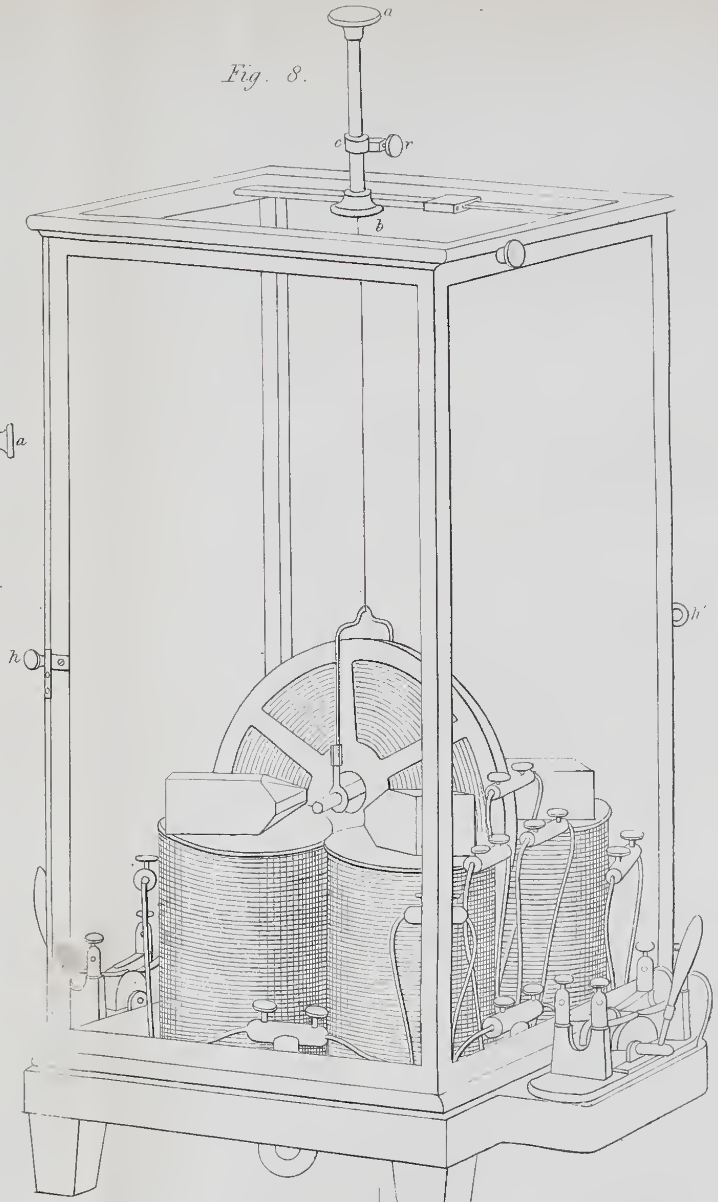


Fig. 8a.

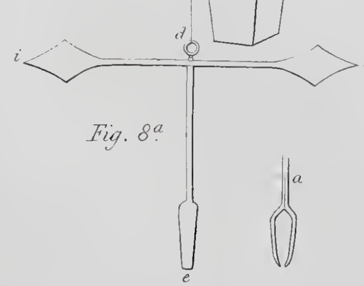


Fig. 4.

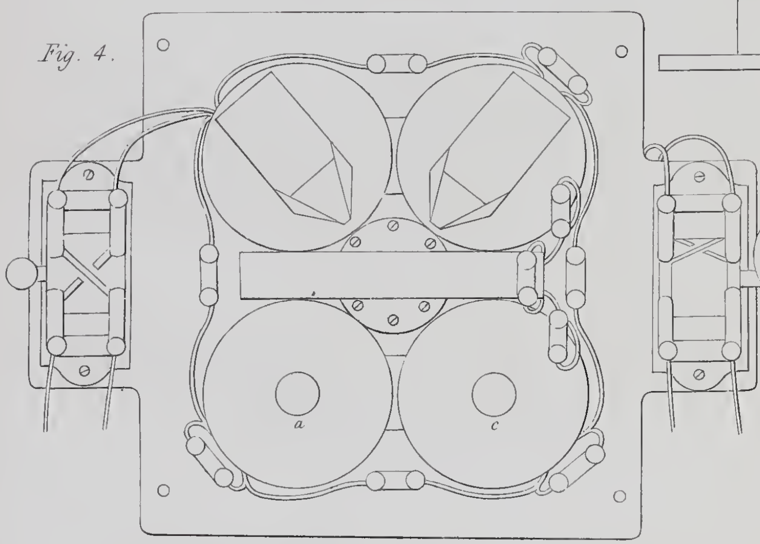


Fig. 5.

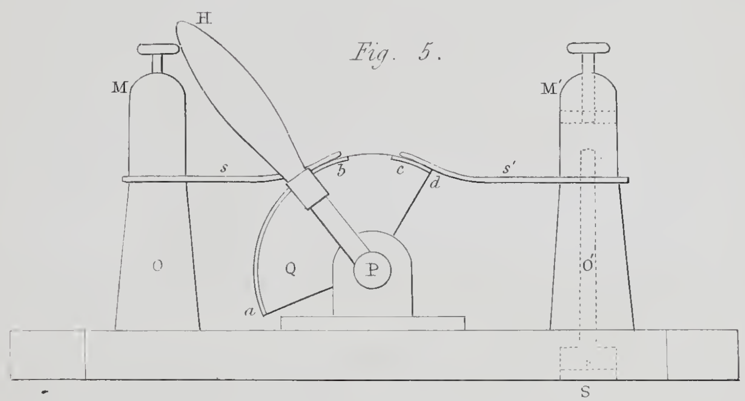


Fig. 3.

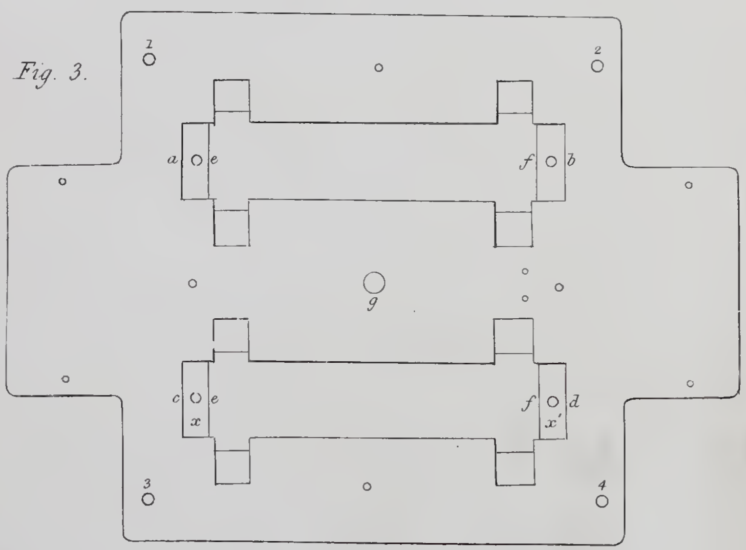
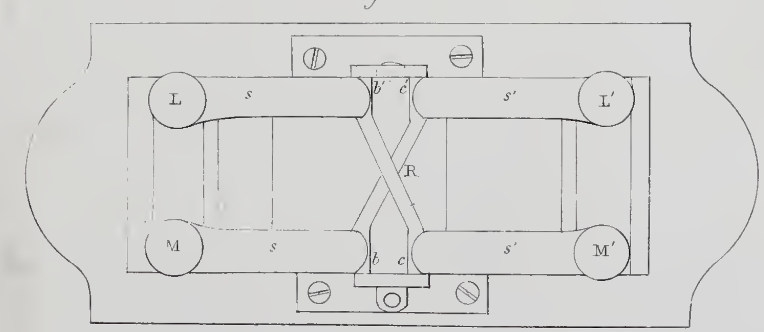


Fig. 6.



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XVII.—THE POLYMAGNET.*

The polymagnet was devised for the purpose of exhibiting before a class of pupils as many as possible of the phænomena of electro-magnetism and diamagnetism.

The instrument consists of an arrangement of two horseshoe electro-magnets, a helix of covered copper wire disposed between them, and suitable means of suspension.

A section of one of the electro-magnets, and its surrounding spirals is given, fig. 1, Plate IV. *ab*, *cd* are two cylindrical cores of soft iron, which are united by a cross-piece of the same material, *ef*. Through the cross-piece pass the strong screws *g* and *h* into the cores, and by them the ends *b* and *d* of the cores, which are accurately planed so as to ensure perfect contact with the cross-piece, are attached to the latter. The diameter of the cores is 1·125 inch, and their distance apart, from centre to centre, 4·85 inches; the cross-piece *ef* is drawn in proportion.

Round each core is a helix of copper wire, overspun with cotton, which was afterwards saturated with shell-lac. In winding the helix, two lengths of wire, one covered with red cotton and the other with green, were laid side by side and coiled as a single wire. The diameter of the wire is 0·1 of an inch, and the weight of it which surrounds each limb of the magnet is 12 lbs. For all four limbs, therefore, a weight of 48 lbs. is made use of.

The second electro-magnet is in every respect similar to the one just described.

Fig. 2 is a front view of a helix of covered copper wire, intended to be placed between the two electro-magnets; it has an internal diameter, *ab*, of 1 inch; an external diameter, *cd*, of 8 inches, and measures along its axis 1·15 inch. The diameter of its wire is 0·065 of an inch, and its weight is 6 lbs.; it is wound so as to form a double coil, as in the case of the electro-magnets.

* Phil. Mag., vol. ix. p. 425.

The radial strips, and central and surrounding ring seen in the figure, are of brass, and hold the coils of the helix compactly together.

Fig. 3 represents a stout slab of mahogany which supports the apparatus. *ab*, *cd* are hollows cut in the slab to receive the cross-pieces of the two electro-magnets; from *e* to *f* the slab is cut quite through, the cross-pieces merely resting on the portions between *f* and *b*, *f* and *d*, &c. The small apertures at x x' show where the screws enter which attach the cross-piece to the slab of wood. The central aperture at g shows where the pin g'' of the helix, fig. 2, enters, the helix thus occupying the central portion of the board. Right and left are two projections for the reception of two current reversers, which will be described immediately. The apertures 1, 2, 3, 4 are for the reception of pins projecting from a glass case intended to cover the whole apparatus.

When the magnets and central helix are fixed in their places and looked down upon, their appearance is that represented in fig. 4; at *a* and *c* the tops of the cores are seen, the movable masses of soft iron which belong to them being removed; the two ends of the other electro-magnet bear two such masses, each formed from a parallelopiped 4·5 inches long, 2 inches wide, and 1·25 inch high, having one end bevelled off so as to render it pointed, the other end being suffered to remain flat. The distance between those movable masses may be varied, or the body to be examined may be suspended either between *surfaces* or *points*, according to the nature of the experiment. The projections of the current reversers upon the horizontal plane are seen right and left.

Simplicity and efficiency being the objects aimed at, a current reverser was devised which fulfils these conditions. A front view of the instrument is given in fig. 5, and its horizontal projection in fig. 6. Q is the section of a quadrant of wood, which is capable of being turned by the handle HP; *ab* is the section of a strip of brass laid on the periphery of the quadrant; *cd* is a shorter strip similarly laid on; between *b* and *c* is a gap, formed of the wood of the quadrant itself, or of a piece of ivory or glass inlaid; *s* and *s′* are two brass springs,* which are shown in the figure to rest upon the strips of brass *ab* and *cd*; M M′ are two

* Copper, I think, would be better than brass.

clamps secured to the wooden pillars O and O′ by screws S, which pass up through the latter. The plan below corresponds to the section above. From *b*, fig. 6, the strip of brass crosses to *c*′, and from *c* to *b*′, both being insulated from each other at R. Supposing, then, the two clamps M and L to be connected with the two poles of a galvanic battery, the current entering at M would flow along the spring *s* to *b*, thence to *c*′, and finally along the spring *s*′ to the clamp L′: in like manner the current entering at L would attain the clamp M′. In this position of things the handle of the instrument leans to the left, as in fig. 5. If the current is to be interrupted, this is secured by setting the handle vertical; for when the handle is in this position, the spring *s*′ rests upon the non-conducting surface *bc*, and the circuit is broken. If it be desired to send the current direct from L to L′, and from M to M′, this is accomplished by causing the handle to lean to the right; when this is done, both the springs *s s*′ rest upon the self-same strip of brass *ab*, and there is direct metallic communication between L and L′, and between M and M′. This reverser has been tested practically, and found very convenient. It is very similar to an instrument devised by Professor Reusch, but simpler and more easily constructed.

Fig. 7 is a plan of the top of the glass case which surrounds the polymagnet. At C a brass tube is cemented to the glass, which is here perforated, and through the tube a rod passes furnished at its lower extremity with a hook, to which is attached a suspending fibre. *ab* is a horizontal brass cylinder capable of being turned on its axis by the milled head at *a*, and thus coiling a suspending fibre around a groove marked at *d*: the cylinder is also capable of sliding right and left, so that the body suspended from the fibre may be moved laterally, and the amount of motion measured on the graduated bar above.* *a′b′* is another horizontal suspension rod, in every respect similar to the former.

The whole instrument, surrounded by its glass case, is shown in perspective in fig. 8. The magnets are visible, with the moveable masses of soft iron resting upon them; in the centre is seen the helix sketched in fig. 2, and within the helix a bismuth bar supported by several fibres of unspun silk attached to the central

* This arrangement, though very convenient for private research, is not necessary for lecture experiments.

rod which passes through the top of the glass case. The manner of suspension of the bismuth will be understood from the drawing, certain practical artifices which suggest themselves when the drawing is attentively inspected being introduced to facilitate the placing of the axis of the bar along the axis of the surrounding helix. The current reversers are seen *without* the case; two opposite sides of the latter can be opened by the handles h and h', so that free and easy access to the interior is always secured.

Experiments to be made with the Polymagnet.

1. All the experiments that are usually made with an upright electro-magnet.

2. The various portions of the instrument may with great facility be lifted separately out of the case. One of the electro-magnets being thus removed, a rope can be passed through a ring r, introduced for this purpose into the cross-piece, fig. 1: adjacent to the screws g and h two plates of brass are seen; these are attached to the brass reels of the helices, and by passing a pin through the holes shown in the figure, the helices are prevented from falling when the magnet is turned upside down. Attaching the rope to a hook in the ceiling, or to a strong frame made for the purpose, experiments on the lifting power of the magnet may be made.

3. While one of the magnets is suspended as last described, the other, which is of exactly the same size, can be brought up against it, the free ends of the four cores being thus in contact. The same currents being sent through both magnets, we have the mutual attraction of two electro-magnets instead of the attraction of an electro-magnet for a mass of soft iron, as supposed in the last experiment. The arrangement just described is indeed precisely that devised by M. Pouillet in the construction of a powerful electro-magnet for the Faculty of Sciences at Paris. To the cross-piece of the second magnet a ring is also attached, from which weights can be suspended.

4. The cross-pieces may be removed by withdrawing the screws g and h, and the spirals may be made use of singly with their corresponding bar-magnets. As two wires surround each coil, one of them may be used to exhibit the induced currents developed by the other. The phænomena of the extra-current

may also be studied, and the remarkable effect produced by connecting the two ends of one of the wires, or the spark of the extra-current in the other, may be exhibited.

The milled head *a*, fig. 7, can be screwed off, and the rod *ac* pushed downwards into the case; the helix in the centre can also be readily lifted out of its position and removed from the glass case—we will suppose this done. The two electro-magnets alone are now within the case, and the view is uninterrupted, which would not be the case if the helix had been permitted to remain.

The fibre hanging from the groove *d*, fig. 7, can be so arranged that any substance attached to it shall hang between the moveable masses of soft iron which surmount the electro-magnets, and the same arrangement can be made for the fibre suspended from the groove *d′*.

But a body suspended between the movable masses of soft iron would be hidden by these masses from the audience, and hence to render the motions of the body visible the following expedient was adopted:—Fig. 8*a* represents a thin index of ivory about 4 inches long, and shaped as in the figure; from the centre depends the stem *dc*, which is terminated by a tongs-shaped arrangement which can clasp the body to be submitted to experiment; to the right of the index a section of the little ivory pliers, by a plane passing through the stem at right angles to *i n*, is given; the stem is slit up to *a*, so as to allow the pliers being opened to receive the body to be examined, which they then clasp in virtue of the elasticity of the ivory. The stem *de* is of such a length, that when the body is in the centre of the space between the poles, the index *i n* is seen above them; and, as the index follows all the motions of the body underneath, these motions are recognised by all who see the index.

5. If an ordinary magnetic bar, sufficiently feeble, be suspended between one pair of poles, and an ordinary diamagnetic bar between the other pair, on sending the same current round both magnets, the index of the former sets itself parallel to the polar line, while the index of the latter sets itself perpendicular to the polar line, and thus the phænomena of magnetism and diamagnetism address the eye simultaneously.

6. In the same way, if a normal magnetic bar be suspended between one pair of poles, and an abnormal magnetic bar

between the other, the antithesis of their deportment may be made manifest. The same antithesis is exhibited when we compare a normal diamagnetic bar with an abnormal one.

7. And when between one pair of poles is suspended a normal magnetic bar, and between the other pair an abnormal diamagnetic one, the apparent identity of deportment of both bars is rendered evident at once. The same identity is shown when we compare the abnormal magnetic bar with the normal diamagnetic one.

8. Causing the points to face each other, instead of the flat ends of the poles, and observing the directions given in the paper spoken of, the curious phænomena of rotation on raising or lowering the body from between the points, first observed by M. Plücker, and explained in the paper referred to, may be exhibited.

9. To show that a bar of bismuth, suspended within a helix and acted upon by magnets, presents phænomena exactly analogous to those of soft iron, only always in opposite directions, let the flat helix be replaced between the two electro-magnets. The bar of bismuth used in experiments with the instrument now described is 6 inches long and 0·4 of an inch in diameter. Suspended so as to swing freely within the helix, its ends lie between the movable masses of iron which rest upon the electro-magnetic cores. Four poles are thus brought simultaneously to bear upon the bar of bismuth, and its action is thereby rendered both prompt and energetic. The two poles to the right of the bar must both be of the same name, and the two to the left of the bar of the opposite quality. If those to the right be both north, those to the left must be both south, and *vice versâ*. On sending a current from 10 or 15 cells round the helix, and exciting the magnets by a battery of 4 or 5 cells, the current reversers place the deflections of the bar entirely under the experimenter's control. By changing the direction of the current in the helix by means of its reverser, a change of deflection is produced; the same is effected if the polarity of the magnets be changed by the reverser which belongs to them. For a full description of all these phænomena I must refer the reader to the paper on the nature of the diamagnetic force already mentioned.

10. To those acquainted with what has been done of late

years in diamagnetism, numerous other experiments will suggest themselves. The antithesis of two isomorphous crystals, one magnetic and the other diamagnetic, the general phænomena of magnecrystallic action, and the analogous effects produced by pressure, may all be exhibited.

By placing one of the helices of the electro-magnet upon the other, a coil of double length is obtained, and two such coils may be formed from the four which we have described. For the additional expense of the iron merely, a single electro-magnet, far more powerful than either of the others, because excited by twice the quantity of coil, may be obtained.

I think it would be an improvement if the suspensions were independent of the glass case, so as to permit of the entire removal of the latter. The best way of showing the deflection of the bismuth bar within the central helix to a large audience, is to attach a long, light index to the bar itself, and permit this index to enter a French shade which will protect it sufficiently from currents of air. With this arrangement the motions are strikingly evident, and may be seen by hundreds at once. The instrument above described was constructed by Mr. Becker, of Newman Street, and its cost is about twenty-four pounds. It was not my intention originally to have so much wire round the electro-magnets; and the effects may also be made manifest with a smaller central coil. I have no doubt that with 8 lbs. of wire round each limb of the electro-magnets, and a central coil weighing 4 lbs., the experiments might be exhibited to a large audience with perfect distinctness. A sensible diminution of cost would of course accompany this diminution of material and labour.

XVIII.—ON MAGNUS'S INVESTIGATION OF THERMO-ELECTRIC CURRENTS.*

Exactly thirty years have flown by since the discovery of thermo-electricity by Seebeck in Berlin. Since that time our knowledge of facts in connection with this subject has been enriched by the labours of Becquerel, Sturgeon, Matteucci, Henrici, and others; but our advance towards principles has been slow. Indeed, some of the facts at present generally accepted are of so incomprehensible a nature; the results of various experimenters—and even of the same experimenter at different times—are so perplexing and contradictory, as pressingly to indicate the necessity of further and stricter examination. In the production of thermo-currents and the determination of their directions, so many hidden influences come into play, that if one subject more than another require the exercise of patience and experimental tact it is this. Until very lately every attempt at progression in this department of inquiry was accompanied by the unpleasant conviction that there was no sure starting-point; and hence he that would advance had to begin afresh, and jealously test every result of his predecessors. This is the state of things which the investigation of M. Magnus is intended to remedy, and his memoir on the subject furnishes internal evidence of the precision with which the inquiry has been conducted. The investigation is far from exhausting the subject, but it lets us know precisely where we are; new and striking facts have been added, errors have been corrected, anomalies accounted for, and the first great step made towards the reduction to law of phænomena which have hitherto perplexed philosophers.

* Philosophical Magazine, vol. iii. p. 81.

The wire usually applied in the construction of galvanometers often presents a difficulty in enquiries like the present. That purchased at the merchants is so magnetic as greatly to interfere with the purity of the experiments. To obviate this defect, some precipitated copper was obtained from a galvano-plastic manufactory; but the metal, after having been cast into cylindrical moulds, was found so magnetic as to necessitate its rejection. The pure metal was finally obtained in the following manner: an excess of ammonia was added to a solution of sulphate of copper, the precipitated oxide being thus redissolved, and the iron mixed with the salt separated; the solution was filtered, evaporated to dryness, and the ammonia expelled; the sulphate thus procured was redissolved in water and precipitated by the voltaic current. This metal, however, was exceedingly brittle, and required to be melted eight times in succession before it could be drawn into wire; when drawn, however, it was found to answer its purpose perfectly.*

In the following pages we shall often have occasion to speak of the direction of the current, and it is therefore prudent to define clearly in the first instance what is meant by this expression. If a strip of copper and a strip of zinc be immersed in a conducting fluid, and the exposed ends be united by a copper wire, the current is said to proceed from the copper through the uniting wire to zinc, and from the zinc through the fluid to the copper. Supposing a bit of antimony to be put in the place of the copper, and a bit of bismuth in the place of the zinc, and doing away with the fluid, that the free ends of both are brought into contact and the place of contact heated; the consequent thermo-electric current will act upon a magnetic needle exactly like that generated by the zinc and copper pair. The current therefore passes from antimony *through the wire* to bismuth (from A to B), but from bismuth to antimony (against the alphabet) *across the place of junction*. Whenever it is stated in this Report that the current passes from one metal to another, the words 'across the place of junction' are always implied.

It was soon ascertained by Magnus that a difference in point

* The magnet is an admirable test of the fitness of a wire for a galvanometer. If the wire be feebly repelled it is all right; if attracted, it ought to be rejected. See the commencement of a paper 'On the Absorption and Radiation of Heat by Gases and Vapours'—Phil. Trans., 1861, and Phil. Mag., September 1861.—J. T., 1870.

of hardness was sufficient to give rise to a current. When a portion of a wire rendered hard by drawing was heated to redness and thus softened, on warming the junction of hard and soft, a current was always obtained. In like manner, when a portion of a soft wire was rendered harder by hammering, a current was produced on heating the junction of hard and soft.

For these experiments a particular arrangement of apparatus was devised; and to prevent any new change in the structure of the wire, it was rarely heated beyond the temperature of boiling water. Particular care was also taken to preserve the points where the two ends of the wire experimented with joined the wire of the galvanometer at the same temperature, a condition absolutely necessary to prevent the formation of currents at these points of junction.

M. Becquerel was the first to demonstrate, that when a wire is knotted and heated in the vicinity of the knot, a current is exhibited. As, however, M. Becquerel employed a red heat in his experiments, it is possible that the current obtained was due to a softening of a portion of the wire, while the knotted portion retained its hardness. Such a result is still more probable in the case where the point of junction of a thick and thin wire is strongly heated. Up to the present time it has been an accepted fact, that a difference in point of thickness merely is sufficient to originate a current.

For the stricter examination of this question two semi-cylinders of brass were provided, and along the axis of each a semi-cylindrical hollow was worked out from end to end. Into this hollow a brass wire was accurately fitted; so that when one piece was placed upon the other, the whole had the appearance represented in Plate III. fig. 1. The slightest heating of the wire in the neighbourhood of its thick case was sufficient to develope a current, and the currents developed when the wire was heated at both ends of the case in succession were in opposite directions.

What is the proximate source of the electricity in this case? Is it the result of a mere difference of thickness; is it to be referred to a difference in chemical composition; or is it due to a difference of hardness between the wire and its encompassing cylinder? If a piece of metal be laid upon another piece of a different metal, in the manner represented in fig. 2, when the

Plate 9.

point *c* is warmed a thermo-electric current is evoked, which circulates for the most part within the boundaries of the two pieces. If, however, the extreme ends of the bar be united with a galvanometer, a branch current will exhibit itself; hence if the thin wire and its encompassing sheath be not perfectly homogeneous, something similar may be expected to take place.

To decide this question, the following experiments were executed:—A brass wire 6 feet long was encircled by a number of pieces, each 1 foot in length, cut from the same piece as the 6-feet wire and placed parallel to it; the short pieces were tied round the long wire by a non-conducting thread. When the portion of the wire adjacent to the surrounding bundle was heated, *no current was observed*; the experiment was repeated with a second wire, but with the same result.

A brass wire 3 feet long and 3 lines in thickness was so reduced, that a length of 6 inches of its central portion had a diameter of only half a line (see fig. 3). Both of the points *g* were heated in succession, but in neither case was a current exhibited. Eighteen inches of another brass wire of 3 lines diameter were reduced to 0·7 of a line, and each end of the portion thus reduced was screwed into a piece of the thick wire from which it had been taken; on heating the place of junction of thick and thin there was no current. Again, part of a piece of brass wire 3 lines in thickness was drawn out until a diameter of half a line was obtained; both thick and thin portions were then heated to redness, and the oxide carefully removed from the surface; their ends were laid one upon the other and thus heated, but no current was observed. In all these cases care has been taken to have the thick and thin portions homogeneous; and we see that when this point is secured, a current *never exhibits itself*. The mere difference in point of thickness is therefore not sufficient to originate a current, as heretofore believed. M. Magnus explains the knot experiments of M. Becquerel by reference to the fact, that the structure of the wire was first altered by heating it to redness. If the temperature applied do not exceed 100° C., no current is ever observed.

It has also been affirmed, that the production of a thermo-electric current is in some measure dependent on the radiative power of the metals employed. A German silver wire was covered by galvanic precipitation with a coating of copper

throughout a portion of its length; the wire was heated at the place where the coating ceased, and a tolerably strong current was the consequence. Was this result due to the contact of chemically different metals, or to a difference in the radiative powers of both? The wire was covered with various non-conducting substances, such as soot, gutta percha, wood, &c.; but when the point where the coating ceased was heated, no current was observed. In like manner, when one portion of a wire was finely polished, and an adjacent portion rendered rough by sand-paper or by the file, on heating the junction between the rough and smooth there was no current, although the radiative powers of both portions must have been different. It is thus proved that a difference in respect to radiative power is not sufficient to originate a thermo-electric current.

It has been already stated, that where a difference in point of hardness exists, a current is produced. To examine this point further, a number of wires each 6 feet long and 0·45 of a line in thickness were chosen; and of those which could bear a high temperature, two feet in the middle were heated to redness and thus rendered soft. Of the more fusible metals, such as tin, lead, zinc, &c., two feet were heated in an oil-bath at 200° C. for an hour. When cooled, the two ends of each wire were united with the galvanometer; one of the junctions between hard and soft was heated, and the consequent current observed. The following table exhibits the results of these experiments: it will be observed that the direction of the current does not preserve a constant relation to hard and soft. In some cases it flows from soft to hard, in other cases in the opposite direction.

Name of metal	Direction of current	Deflection
Brass	From soft to hard	55°
Silver (pure)	do.	46
Steel	do.	45
Silver with 25 per cent. copper	do.	40
Cadmium	do.	25
Copper	do.	18
Gold No. 1, with 9·7 per cent. copper	do.	10
Platinum	do.	5
Gold No. 2, with 2·1 per cent. silver	do.	2
German silver	From hard to soft	34
Zinc	do.	30
Tin	do.	5
Iron	do.	4
Lead	Uncertain	

By means of the pretty little instrument represented in fig. 4*, and which its inventor has named the monothermic pile, the action may be considerably increased. A length of hard brass wire is taken, every alternate six inches of which are rendered soft by heating to redness. Thus six inches of soft wire succeed six inches of hard throughout the entire length. The wire is then wound round a frame of suitable size, and presents when wound the appearance of a rectangle, two of the opposite sides of which are composed of hard and soft wire respectively; the centres of the other two sides are the junctions of hard and soft. The two ends of the wire being connected with the galvanometer, if either the hard side or the soft side be heated we have no action; but if one of the junctions of hard and soft be taken between the finger and thumb, the heat of the hand is sufficient to cause a deflection of 90 degrees. The writer has to thank Prof. Magnus for an instrument of this kind. The wire presents the same uniform appearance throughout, and to an observer ignorant of the process to which the wire has been subjected, the deportment is exceedingly striking and enigmatical.

If two wires of the same material be taken, and if one be heated, on causing the hot and cold wires to touch each other a current is observed. This is modified if the one wire be hard and the other soft: sometimes the difference of temperature and difference of hardness work together and increase the current by their united action; sometimes they oppose each other, and a decrease of the current is the consequence. This matter has been investigated very fully. It will perhaps be well to describe beforehand the manner in which the experiments were made.

In a tin cylindrical vessel, AB, fig. 6, two tubes, *ab* and *cd*, crossing each other at right angles, were introduced; each tube had a diameter of half an inch; from *f*, where the tubes crossed, another vertical tube abutted upwards and passed through the cover of the vessel; the three tubes communicated with each other inside; through one of the horizontal tubes the wire to be heated was introduced and fastened by corks at *a* and *b*; to prevent metallic contact, all three tubes were lined by smaller ones of glass; at *f* the wire was exposed, and rested upon a flat

* Another form of this instrument is represented in fig. 5.

piece of wood introduced beneath it; in the vertical tube was a wooden rod which nearly filled it, but which could be moved through the tube with freedom; the rod carried at its end a pound weight of lead, P; the cylindrical vessel was filled with water and kept constantly boiling, and as soon as it was certain that the wire within had assumed the temperature of boiling water, the wooden rod was raised, and the cold wire was introduced crossing the warm one; this being effected, the rod was permitted to descend, and the wires were pressed together by the weight P. The following table shows the results of this inquiry. First *both* wires were *hard*, next *both* were *soft*; and finally, the one was hard and the other soft.

One wire 100° C., *the other* 8° C.

Name of metal	Both wires		One hard, the other soft	
	Hard	Soft	The hard warm	The soft warm
German silver	fr. *c* to *w* 40°	fr. *c* to *w* 72°	fr. *c* to *w* = fr. *s* to *h* 5° immediately afterwards fr. *w* to *c* = fr. *h* to *s* 24	fr. *c* to *w* = fr. *h* to *s* 80°
Silver (pure)	do. 7	do. 3	fr. *c* to *w* = fr. *s* to *h* 73	fr. *w* to *c* = fr. *s* to h 68
Copper	do. 3	do. 8	do. do. 24	do. do. 15
Tin	do. 7	do. 10	fr. *w* to *c* = fr. *h* to *s* 7	fr. *c* to *w* = fr. *h* to *s* 20
Zinc	fr. *w* to *c* 28	fr. *w* to *c* 28	do. do. 62	do. do. 34
Platinum	do. 24	do. 22	do. do. 13	fr. *w* to *c* = fr. *s* to *h* 36
Gold No. 2, with 2·08 p. c. silver	do. 5	do. 6	do. do. 3	do. do. 5
Gold No. 1, with 9·7 p. c copper	do. 6	do. 5	do. do. 2 immediately afterwards fr. *c* to *w* = fr. *s* to *h* 11	do. do. 19
Cadmium	do. 26	do. 15	do. do. 53	do. do. 55
Brass	do. 3	do. 12	do. do. 90	do. do. 90
Silver, with 25 per c. copper	do. 6	do. 12	do. do. 82	do. do. 78
Mercury	do. 0	0	0	0
Lead	...	...	uncertain	

w signifies warm; *c*, cold; *h*, hard; *s*, soft.

The discussion of this table would give rise to some interesting speculations, which, however, I forbear dwelling upon, as M. Magnus himself has not thought proper to introduce them—doubtless because he considers the subject not yet ripe for such. Let us hope, however, that results so suggestive will receive at the hands of their discoverer the development of which they seem capable.

Experiments such as these are always valuable as points of reference; I therefore introduce a second table, in which a temperature of 250° C. was applied.

One wire 250° C., *the other* 8° C.

Name of metal	Both wires		One hard, the other soft	
	Hard	Soft	The hard warm	The soft warm
Silver (pure) .	fr. *c* to *w* 20°	fr. *c* to *w* 17°	fr. *c* to *w* = fr. *s* to *h* 90°	fr. *c* to *w* = fr. *h* to *s* 3 immediately afterwards fr. *w* to *c* = fr. *s* to *h* 90
Platinum . .	fr. *w* to *c* 84	fr. *w* to *c* 80	fr. *w* to *c* = fr. *h* to *s* 90	do. do. 90
Gold No. 2, with 2·08 p. c. silver	do. 17	do. 28	do. do. 12	do. do. 27
Gold No. 1, with 9·7 p. c. copper	do. 54	do. 31	do. do. 10 immediately afterwards fr. *c* to *w* = fr. *s* to *h* 30	do. do. 69
Silver with 25 per c. copper .	do. 90	do. 90	fr. *w* to *c* = fr. *h* to *s* 6 immediately afterwards fr. *c* to *w* = fr. *s* to *h* 90	do. do. 90
Mercury . .	0	0	0	0

A variety of notions entertained by physicists as to the origin of thermo-electric currents have been already mentioned. M. Magnus also discusses the hypothesis, that the cause is to be sought in the unequal decrease of temperature on both sides of the place heated, and the notion that they are to be referred to a difference of thermal conductivity on the part of the metals employed. He dissents from both these views; and proves, in the following manner, that the conductivity of the hard wire was in no way different from that of the soft one.

From a stout brass wire 2·25 lines in diameter, rendered quite hard by the act of drawing, two pieces each 4 feet long were separated. One of these was heated to redness, and thus rendered soft; both wires were then brought into the tin vessel already described and there subjected to the same temperature; the ends of the wires without the vessel were at such a distance from it, that they retained the same temperature; to one end of the galvanometer wire a bar of antimony was attached, and to the other end a bar of bismuth, both being bevelled off to an edge; the edge of one of these bars was laid upon the soft wire, and the edge of the other upon the hard wire; when a difference of temperature existed between the

points of contact, a current was exhibited at the galvanometer; when the temperatures were alike, no current was generated. By finding points of equal temperature in this manner, and by measuring the distance between these points on each wire, their respective conductive powers were ascertained. It was found that the conductivity of both was the same. The conclusion finally arrived at by M. Magnus is, that the currents are produced by the contact of unhomogeneous metals.

In connexion with this subject, M. Svanberg has made an interesting communication to the Academy of Sciences at Paris, from which the following is extracted:—In large masses of bismuth and antimony the crystalline texture is never in all parts the same, but it is not difficult to find some homogeneous portions. From these little bars may be formed, the length of which may be at various inclinations to the planes of crystallisation.

Among the planes of cleavage of these two metals in a crystallised state, there is one, which, as Mr. Faraday was the first to observe, is distinguished from all others by its superior brilliancy. This plane is perpendicular to the crystallographic axis. Among the other planes there is one which does not fall far short of the above in point of brightness. Let the bars whose length coincides with the intersection of those two planes be named (A), and those bars whose length is perpendicular to the plane of most eminent cleavage be named (B).

In the case both of bismuth and antimony the bars (A) are more positive, and the bars (B) more negative, in the thermo-electric series, than any other bar which can be formed of the same metal. The thermo-electric force between the antimony (A) and the antimony (B), and between the bismuth (A) and the bismuth (B), is pretty considerable. If a bar intermediate between (A) and (B) be taken, that is to say, such that the direction of its length is otherwise inclined to the plane of most eminent cleavage, or if it do not possess a regular crystalline texture, such a bar is negative with (A) and positive with (B).

This variability in the thermo-electric power of bismuth and antimony seems to furnish a key to the explanation of the currents observed by Seebeck, Sturgeon, and Matteucci, in circuits formed of a single one of these metals.

With regard to the direction of the currents between warm bismuth and the cold bismuth, the warm antimony and the cold antimony, different experimenters have arrived at different results. Vorsselmann de Heer, the last who has occupied himself with this subject, has observed the current to pass sometimes from the cold to the warm metal, and at other times from the warm metal to the cold. He concluded from his observations, that the direction of the current depends on the greater or less difference of temperature between the two bars. These cases of reversion exhibited themselves in a special manner with antimony.

That such experiments should have any value, it is absolutely necessary that the bars made use of should occupy the same place in the thermo-electric series. Thus, for example, we must compare (A) with (A), and (B) with (B), but not (A) with (B). In the first place, it ought to be ascertained whether the two bars be absolutely homogeneous. It is a remarkable fact, that the deportment of (A) towards (A) is not the same as that of (B) towards (B).

M. Svanberg's mode of experimenting was as follows:—The two bars were fixed in copper handles, and these were connected with a very sensitive galvanometer. Up to the point of contact with the copper, the bars were enveloped in snow almost to the free extremities. In this case, when the extremities are brought into contact and then heated to any temperature whatever, there ought to be no current; and this furnishes a test as to whether the bars are thermo-electrically homogeneous. But if *before* bringing the bars into contact, the end of one of them be either heated or cooled, a current is observed, the direction of which is indicated by the galvanometer. If the two bars be of the bismuth (A) or of the antimony (A) the current proceeds from the cold to the warm metal; with the bars (B), however, the direction of the current is the opposite, it passes from the warm metal to the cold. This result is exceedingly remarkable, but it has been proved by multiplied experiments.

Another memoir on this subject by M. Franz of Berlin has recently appeared in Poggendorff's *Annalen*. He uses cubes of bismuth. The cubes are placed between two small copper pillars connected with a galvanometer; the pillars are moveable, and thus permit of the cubes being pressed together. We will call

the direction from pillar to pillar the axial direction, and that perpendicular thereto, the equatorial. In some cubes the plane of most eminent cleavage formed two of the opposite sides, and in some the said plane was inclined at an angle of 30° or 60° to two opposite sides. When two of the former were so placed that the cleavage *throughout both* stood either axial or equatorial, no current was observed on heating. When the cleavage of one cube was axial and that of the other equatorial, there was a deflection of 45°. When a pair of the other cubes were placed so that the cleavage of each made an angle of 30° with the plane of the horizon, a current of 30° was observed; when the angle with the horizon was 60°, the deflection was 19°·7. Bismuth was also found to change its thermo-electric power in contact with other metals, when the position of the plane of most eminent cleavage in relation to the plane of contact of both metals was altered. These results appear to stand in intimate connexion with those of M. Magnus.

Application of the results of M. Magnus to certain effects observed by M. Regnault.

An exceedingly interesting memoir, 'On the Measurement of Temperatures by Thermo-electric Currents,' by M. Regnault, appears in the 'Philosophical Magazine' for June 1850. In the course of experiment some very perplexing and indeed unexplainable phenomena presented themselves, the solution of which appears to be furnished by the experiments of M. Magnus. This does not appear to have been noticed by the latter philosopher, as he is silent on the subject. I have carefully plotted the seven series of results given by M. Regnault; taking the difference of temperature of iron and platinum as abscissæ, and the difference between bismuth and antimony as ordinates, and using a horizontal scale of twenty, and a vertical scale of ten divisions to an inch. In the curves formed by the plotting of the last three series, where every pains was taken to remove all possible causes of disturbance, the anomalies are most striking. Laying the datum line of one upon that of another, and commencing at a common point, the curves ought to superpose; but

they do not; that derived by plotting the 5th series falls considerably below those obtained by plotting the 6th or 7th. A mere inspection of the table exhibits the same in particular cases. For example, a difference of temperature of 268°·64 between iron and platinum, corresponds in the third series to a difference of 13°·71 between bismuth and antimony; whereas in the 6th series, a difference of 268°·66 between the former corresponds to a difference of 17°·77 between the latter; and in the 7th series, a difference of 268°·56 is equivalent to one of 18°·60. It hence appears that the thermo-electric force of iron and platinum is relatively greater in the 6th and 7th series than in the 5th. We shall now endeavour to account for this hitherto inexplicable result. Turning to the table at page 85 of this Report, we observe that the current formed at the junction of hard and soft in an iron wire passes from hard to soft, which proves that the iron is rendered *more negative* when it is softened by heat. Let us now devote a moment's attention to the result with platinum wire at page 87. In the case of two homogeneous wires, the current passes from warm to cold, causing a deflection of 24° when both wires are hard. When a hard and soft wire are taken, and the former is heated, the current passes as before from warm to cold, causing, however, a deflection of only 13°. It thus appears that the soft wire is less negative, or, what is the same, more positive than the hard wire. Consistently with this, if the heated wire be the soft one, the fact of its being hot and soft at the same time ought to make the current developed a maximum—this is the case. The deflection observed under these circumstances is 36°.

The general facts being thus established, that iron, when softened by heat, becomes more negative, and that platinum, when softened by heat, becomes more positive, let us apply them to the case before us. M. Regnault commenced his 5th series with a fresh couple of iron and platinum, increasing the difference of temperatures between the hot and cold junctions gradually until it reached 273°·46. The absolute temperature of the hot junction at this point was in all probability 300°. After the couple had been thus heated, it was allowed to cool, and the 6th series was commenced: here the anomaly before alluded to at once presented itself; a certain difference of temperature produced a stronger current than in the 5th series, a result which might

be inferred *à priori* from the foregoing considerations. For the iron by being once heated to 300° has become more negative, as before proved, while the platinum has become more positive; the thermo-electric force of the couple has, in short, been increased, and a more powerful current is the necessary consequence. This is still more strikingly exhibited in the 7th series, where M. Regnault commences with a difference of 103°·80, and goes on increasing to 282°·18; then, without interrupting the series, allows the difference to sink again to 148°·97. The bismuth and antimony equivalent for this is 12°·30; whereas for a difference of 152°·29 between the iron and platinum, *before the difference of temperature had reached the above amount* (282°·18), the antimony and bismuth equivalent is only 11°·69. This fluctuation in the 7th series causes the curve derived from plotting to present somewhat of the appearance of a railway section over undulating ground, whereas in all the other cases it presents a gradual and almost uniform ascent. The 'sudden leaps' noticed by M. Regnault, whose cause he considered it impossible to ascertain, appear to be thus capable of satisfactory explanation.

XIX.—ON KOHLRAUSCH'S VERIFICATION OF THE THEORY OF OHM.*

THE following quotation bears so pertinently upon the subject of the present review, that an apology for its introduction here is scarcely necessary. It is extracted from a discourse by Professor Dove, before the Berlin Society for Scientific Lectures.

'As the (then considered) essential portions of a galvanic circuit were two metals and a fluid, innumerable combinations were possible, from which the most suitable had to be chosen. This gigantic task was undertaken by Ritter, an inhabitant of a village near Leignitz, who almost sacrificed his senses to the investigation. He discovered the peculiar pile which bears his name, and opened that wonderful circle of actions and reactions which, through the subsequent discoveries of Œrsted, Faraday, Seebeck and Peltier, drew with ever-tightening band the isolated forces of nature into an organic whole. But he died early, as Günther did before him, exhausted by restless labour, sorrow, and disordered living. It was soon found that many experiments succeeded better with a single pair of large plates than with several small ones; and, in short, that every apparatus exhibited certain actions better than all others. Here men of science long groped in darkness, when in the year 1827, the theory of galvanism by Ohm, then of Berlin, now of Nürnberg, rose like a pole-star to brighten the obscurity. He showed that, as the apparatus itself was composed solely of conductors, the electric current must proceed not only along the connecting wire from pole to pole, but also through the apparatus itself; that the resistance offered to the passage of the current consisted therefore of two portions, one exterior to the apparatus and one

* Philosophical Magazine, vol. iii. p. 321.

within it. At a stroke, the difficulties which up to this time had beset the subject, and which were thought insuperable by those who had confined their attention to the exterior resistance only, crumbled away.

'Ohm brought forward his discovery in the simple earnest language which distinguishes the true investigator of nature. A theory, he says, which lays claim to immortality must not depend upon the idle garniture of words for the proof of its noble origin, but must show in all its parts, by its simple and complete correspondence with facts, and without the aid of eloquence, its affinity to that spirit which animates nature. The manner in which this theory was received was different in different lands. Henry of Princeton, North America, who at once saw its infinitely practical importance, observes, 'When I first read Ohm's theory, a light arose within me like the sudden illumination of a dark room by lightning.' The Royal Society of London awarded him the Copley Medal, the highest prize given by the Society for physical investigation. In France also the discovery met with the greatest recognition which a foreign investigator could expect there. But what reward did Ohm reap in Germany? While the most laborious empirical enquiries were instituted, among which those of Fechner in Leipsic deserve especial mention, to bring the theory in all possible ways to the touchstone of experience, that science whose function it is to think the great thoughts of the Creator over again, glanced down with divine satisfaction from her Olympic throne upon these sublunary occupations. In the Berlin *Jahrbücher für wissenschafliche Kritik*, Ohm's theory was named a web of naked fancies, which can never find the semblance of support from even the most superficial observation of facts; "he who looks on the world," proceeds the writer, "with an eye of reverence, must turn aside from this book as the result of an incurable delusion, whose sole effort is to detract from the dignity of nature."'

The investigations, of which I now purpose giving a review, occupy themselves with the experimental verification of the entire theory of Ohm. A portion of that theory has been already tested by physicists of all lands, and found true: this portion, which on account of its superior importance is called

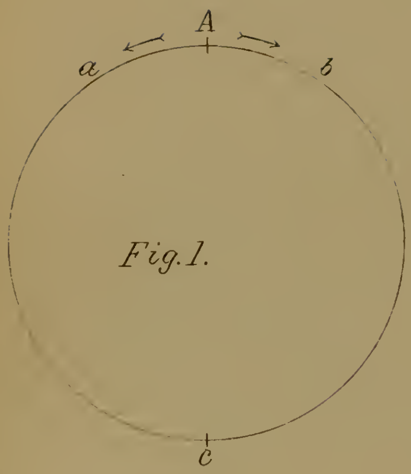

Fig. 1.

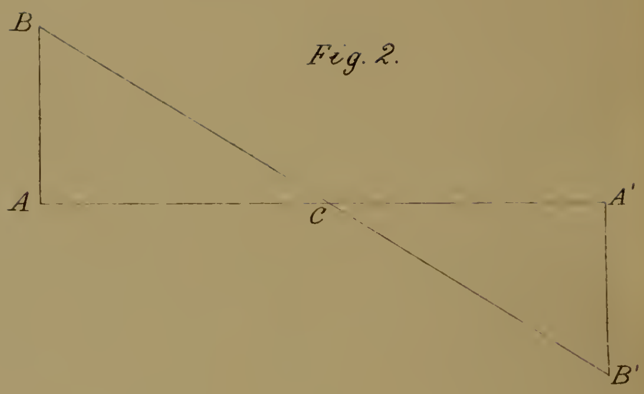

Fig. 2.

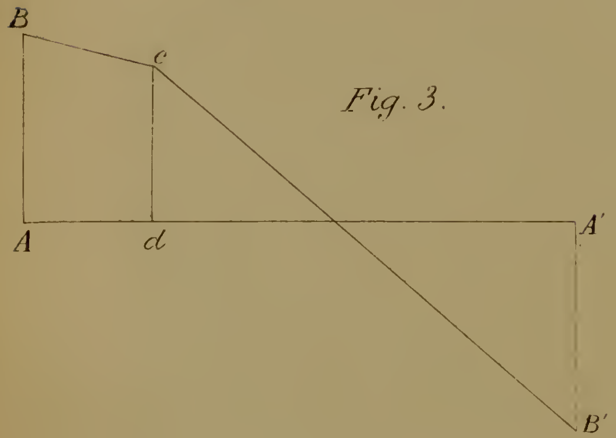

Fig. 3.

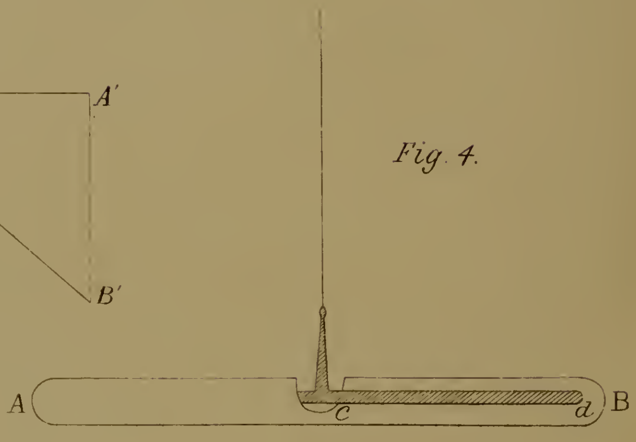

Fig. 4.

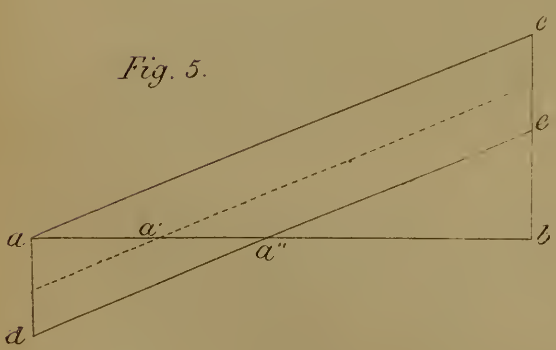

Fig. 5.

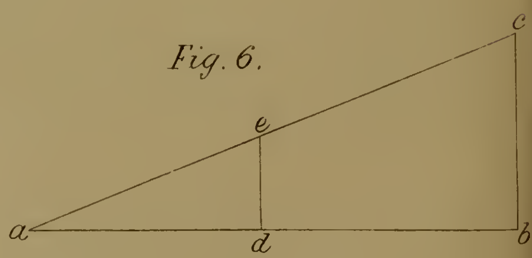

Fig. 6.

the law of Ohm, forms, however, but one link in the chain of causation which the philosopher's speculations place before us. The comparative want of recognition which the other portions of the theory have experienced, is to be chiefly referred to the difficulty of procuring instruments sufficiently delicate to test them experimentally. By the invention and skilful application of suitable instruments, M. Kohlrausch has been able to travel side by side with the speculations of Ohm, and to convert them one after another into experimental facts.

The fundamental portions of Ohm's theory may be briefly sketched as follows :—Let the ring, Plate VI. fig. 1, represent a homogeneous conductor, and let a source of electricity be supposed to exist at A. To fix the ideas, let us suppose an electric machine placed there. The electricity from the machine will diffuse itself over both sides of the ring ; the positive passing towards a, and the negative towards b, both fluids uniting at c. Now if the electricity be so distributed over the ring that a heaping up of the fluid nowhere occurs, then it follows that equal quantities of electricity pass through all cross sections of the ring in the same space of time. If it be assumed that the passage of the fluid from one cross section to another is solely due to the difference of the electric tension at both these points ; and further, that the quantity which passes is proportional to this difference of tension, it follows, that the positive fluid proceeding from A right to c, and the negative fluid proceeding from A left to c, must decrease in tension the further they recede from A.

The tension of the electricity at every point in the circuit may be represented by a diagram. Let the ring be supposed stretched out into a straight line AA′, fig. 2 ; let the ordinate AB represent the tension of the positive electricity, and A′B′ the tension of the negative electricity at the point of excitation, then the ring being homogeneous and of the same diameter throughout, the straight line BB′ will express the tension for all points of the circuit.

From these considerations, the law of Ohm expressed by the celebrated formula

$$S = \frac{E}{R},$$

where S represents the strength of the current, E the electro-

motive force of the battery, and R the resistance, naturally follows. If the electromotive force AB + A′B′ remain constant, then the greater the length of AA′ the less steep will be the inclination of the line BB′; that is to say, the less will be the difference of tension in two contiguous cross sections. But by the hypothesis, this difference is proportional to the quantity of fluid which passes from one cross section to the other; and hence it follows, that the greater the length of the circuit, the less will be the amount of electricity which passes through any cross section in a given space of time.

If the conductor AA′ be composed of material which offers a greater resistance to the passage of the electricity than that above supposed, as long as its length remains unaltered the distribution of the electricity will be the same. But inasmuch as the moving force, that is, the difference of tension between two neighbouring cross sections, is also the same as before, a less quantity of electricity must pass from section to section in a given time than in the case of the good conductor, that is to say, the current must be weaker. A greater length of the better conductor would produce precisely the same effect. These results find definite expression in the law, that *the strength of the current is inversely proportional to the resistance of the circuit.* Preserving the length and material of AA″ unchanged, and regarding the force AA′ + BB′ as variable, we deduce the law, that *the strength of the current is directly proportional to the electromotive force.*

One additional reference to the manner in which Ohm pictured to himself the electroscopic state of the circuit will suffice. Let the conductor AA′, fig. 3, consist of the same material throughout, but of two portions, possessing different cross sections. Let the cross section of A*d*, for example, be *m* times that of *d*A′; then if equal quantities pass through all sections in equal times, if through a unit of length of wire of *m* times the cross section no more fluid passes than through the thinner wire, the difference of tension at both ends of this unit of length in the former must be only $\frac{1}{m}$th of what it is in the latter. Thus the electric 'fall,' as Ohm terms it, that is, the decrease in the length of the ordinate for the unit of length of the abscissa, will be less in the case of the thick wire than of

the thin, as shown by the line Bc in the figure. The distribution of the electricity in such a circuit will be no longer represented by a continuous gradient, but can nevertheless be easily ascertained by calculation when the electromotive force of the circuit and the cross sections of its different portions are known. If, instead of one wire being thinner than the other, its specific resistance were greater, it would follow from the hypothesis of Ohm, that the greater the resistance of the metal the greater would be the electric fall. The result is summed up in the law, that *the 'electric fall' is directly proportional to the specific resistances of the metals and inversely as their cross sections.*

Thus far we have travelled through a region of pure speculation. To test whether the actual distribution of electricity throughout a galvanic circuit bears any resemblance to that here supposed, an electrometer of surpassing delicacy was necessary. We shall give a brief description of the refined instrument made use of for this purpose by M. Kohlrausch.

A thin needle of silver wire, two inches in length, is suspended horizontally from a glass fibre of exceeding fineness; the fibre which passes in the usual manner through a glass tube is fastened to a torsion-head, the index of which being turned causes the little needle of silver wire at the other end to follow it. The needle lies across a thin strip of silver of its own length, through a slit in the centre of which the needle can descend; at the slit the strip is so bent right and left, that the needle, in following the index, can lay its entire length against the strip. This is the only portion of the instrument which requires a drawing to make it clear; it is represented in fig. 4. AB is the strip of silver, *cd* one-half of the needle crossing the strip in its centre, the other half is hidden by the strip. AB can be raised or lowered, so as to be in contact with the needle or detached from it. When the needle crosses the strip at right angles, the latter is raised so that the needle rests upon it, the apparatus thus forming a continuous cross of conducting material. Electricity, being communicated to the strip, distributes itself over the entire cross; when this is affected, the strip is lowered so that the needle again hangs free. The index above being turned, the needle will be solicited by the torsion of the fibre to approach the strip, but being charged with a like electricity, it will be repelled; by this play of torsion on

the one hand, and repulsion on the other, we arrive at a knowledge of the tension of the electricity communicated. The author has constructed tables from which the electric tension due to any observed amount of torsion can be instantly ascertained.

In connection with the electrometer a condenser was made use of, the accuracy of which was carefully tested beforehand. For experiments with the galvanic circuit, both plates are of brass, suspended in a suitable frame by strings of silk, and separated from each other by three little patches of shell-lac placed at three different points near the periphery. When the poles of the battery are connected with these plates, the one becomes charged with positive, the other with negative electricity; and the strength of the charge is estimated by removing one of the plates to a certain fixed distance, and bringing the other, by means of an isolated copper wire, into connexion with the electrometer.

The electromotive force of a voltaic element, which Ohm expresses in his formula by the letter E, can be variously ascertained: the question suggested itself to Kohlrausch, whether any relation existed between this force and the tension of the electricity of the two poles of the element. The electromotive forces of various combinations were determined by Wheatstone's method. To ascertain the tension at the poles, the circuit, which had been permitted to remain in action for some time was suddenly broken, and the ends of the wires were brought into connexion with the plates of the condenser. The plates were then separated; one of them was immediately brought into connexion with the electrometer, and the strength of the charge was measured. The results derived from this process are contained in the following table :—

Description of element	Electromotive force	Tension at the ends of the broken circuit
1. Zinc and platinum:—zinc in solution of sulphate of zinc, platinum in nitric acid of 1·357 specific gravity	28·22	28·22
2. Do. with nitric acid of 1·213 sp. gr. . .	28·43	27·71
3. Zinc and coal:—zinc in sulphate of zinc, coal in nitric acid of 1·213 sp. gr. . .	26·29	26·15
4. Zinc and copper:—zinc in sulphate of zinc, copper in solution of sulphate of copper .	18·83	18·88
5. *a*. Silver and copper:—silver in cyanide of potassium or common salt, copper in solution of sulphate of copper	14·08	14·27
b. The same afterwards	13·67	13·94
c. The same some time afterwards . .	12·35	12·36

This table establishes the important result, *that the electromotive force is proportional to the electric tension at the ends of the newly-broken circuit.*

The following experiments were instituted to ascertain the electroscopic properties of the active simple circuit. The author considers it practically impossible at present to construct an electrometer which shall directly declare the almost infinitesimal tension which obtains at the various points of the simple circuit, and hence the necessity of calling in the aid of the condenser: the manner in which the instrument was charged is as follows:—

From the lower condensing plate a wire of the same metal as the plate itself proceeded to, and was buried in the earth. A branch was carried from this wire to a point *a* of the closed circuit. When another point, *b*, of the circuit was brought into metallic connexion with the upper plate of the condenser, it became charged to an amount which depends upon the tension existing at *b*, and on the condensing power of the plates. If several such points, *b*, be examined, the charges imparted to the condenser will be proportional to the electroscopic tension at the different points. Instead of connecting the lower plate with the earth, we might connect it and the point *a* directly, and bring the upper plate, as before, into connexion with *b*; experiment proves that the result obtained from this procedure is exactly the same as that obtained by the former method. The mode of observation first indicated is that pursued in the following experiments, the point *a* being deprived of all electric tension by its direct union with the earth.

Experiment 1.—The poles of the element were connected by a long fine wire, which was carried in a zigzag manner from side to side of a light wooden frame, and fastened to the latter by pins; the legs of the Vs thus formed were all of the same length.

a. Any point (*a*) being properly connected with the earth, when another point on that side of *a* from which the positive current proceeded was connected with the upper plate, the latter exhibited positive electricity; when, however, the point lay at the other side of *a*, a negative charge was obtained.

b. As long as the same length of wire existed between the point *a* and the point examined, exactly the same tension was shown by the electrometer, it mattered not in what portion of the circuit the examination took place.

c. When a series of points in the circuit at increasing distances from *a* were examined, the tension was observed to increase, the increase being exactly proportional to the length of wire intervening between *a* and the respective points. Calling to mind what has been said regarding the electric 'fall,' the case before us shows that, in a wire of uniform thickness, the 'fall' is in all places the same.

Experiment 2.—The poles were united by two silver wires of equal lengths but of different diameters; the wires being fused together in the flame of a spirit-lamp, so as to form one unbroken length: it was found,—

a. That in each of the wires the same electric fall existed throughout.

b. When one end of the thin wire was properly connected with the earth and the other end tested, the electrometer showed a charge of the strength E; when one end of the thick wire was connected with the earth and the other end examined, a charge *e* was obtained; the ratio of E : *e* was the same as that of the cross section of the thick wire to that of the thin.

Experiment 3.—The wire connecting the poles was formed of two wires, one of copper, the other of German silver; the former presenting very little resistance to the current, while the resistance of the latter was considerable. The total resistance of each wire was previously ascertained by means of a rheochord. It was found that the entire increase of tension

from one end to the other of the copper wire was to the entire increase along the German-silver wire in the direct proportion of the resistances.

The above results may be summed up as follows:—*In wires of different materials and of unequal thicknesses, the electric fall is directly proportional to the specific resistances of the metals, and inversely as their cross sections*; which is a complete verification of the hypothesis of Ohm.

Experiment 4.—A rectangular wooden trough was constructed, and its interior was coated with wax. At one end was placed a porous cell containing a solution of sulphate of zinc, in which a plate of zinc was immersed; the rest of the trough was filled with a solution of sulphate of copper, and at the opposite end a plate of copper was immersed. The zinc and copper plates were connected by a wire. The edge of the trough was graduated; two copper wires dipped into the solution of sulphate of copper, and by means of the graduation their exact distance asunder could be readily ascertained. One of these wires was well connected with the earth, the other was connected with the upper plate of the condenser. The mode of experiment was, in fact, the same as that pursued with the metallic portion of the circuit. Here also it was found that the tension at the point connected with the discharging wire was zero; right and left from this point a regular increase of tension was observed; on that side from which the current proceeded the electricity was positive, on the other side negative. Further, according to the view of Ohm, who imagined the eleetricity to make its way through the *interior* of both metallic and fluid conductors, the tension at every point in any given cross section is the same. In the case of a metallic conductor it is, of course, impossible to test this experimentally; but in the fluid portion of the circuit, Kohlrausch found exactly the same tension throughout each transverse section, whether he raised or sunk the wire (which in these experiments was everywhere coated with shell-lac except at its extreme end) in the fluid, or pushed it more or less aside laterally.*

I trust the reader bears in mind what has been said regarding

* Weber and Kirchhoff differ from Ohm here. They do not admit a motion of the fluid through the interior of the conductor, but solely along its surface. Their hypotheses, however, lead them to results which entirely agree with Ohm's.

the electric 'fall.' The greater the resistance offered to the passage of the current, the greater the fall. In a thin wire, the line expressing the tension at every point will be a steeper gradient than in a thick wire; and in the fluid portion of the circuit the gradient may be expected to be steeper than in either of the former cases, for here the resistance is greatest. The simplest possible circuit must therefore exhibit a series of gradients expressive of the tension of its various parts. There is the fall along the connecting wire, the fall along the zinc and copper plates (which, however, is practically zero, as they offer almost no resistance), and the fall along the fluid. But let us suppose the resistance in every portion of the circuit to be referred to a certain unit, and that the distances along the datum line from which the tensions are plotted are measured off with reference to this unit; that, for example, if an inch of the fluid portion exhibit a fall three times as great as an inch of the solid portion, the said fluid portion shall, on the datum line, be expressed by a distance three times as great as that which expresses an equal length of the solid portion; it is evident that when the resistances are thus referred to a common standard, the line which expresses the tension must be one uniform gradient from beginning to end. Ohm calls the length of a circuit referred to such a standard its *reduced length*.

It has already been stated, that when any point of the circuit is perfectly discharged, the tension at this point is null, and increases in tension right and left, showing positive electricity on that side of the point from which the current proceeds and negative electricity at the other side; the length of the circuit which shows the one fluid or the other will depend upon the position of the point; if exactly central, as at a'', fig. 5, the lengths will be alike. If the point be nearer to the zinc pole than to the copper pole of the arrangement, as at a', the length of wire exhibiting positive electricity will be greater than the length exhibiting negative electricity; and if the point be chosen contiguous to the zinc plate, as at a, the whole circuit will exhibit positive electricity.

Having the electromotive force bc, and the reduced length of the circuit, we are taught by the theory of Ohm to deduce by simple calculation the electroscopic state of every single point.

Let the scheme in fig. 6 represent the state of things in a circuit where the discharged point a is contiguous to the zinc pole. The reduced length, ab, and the electromotive force, bc, being given, let d be any point whose tension, de, we wish to ascertain. Let $bc = a$, $de = u$, $ab = l$, $ad = \lambda$; then by similar triangles,

$$u : a = \lambda : l, \text{ or } u = \frac{\lambda}{l} \cdot a;$$

or, expressed in words, if the reduced length of the circuit between the discharged point and the point whose tension is sought be divided by the reduced length of the entire circuit, the quotient, multiplied by the electromotive force, gives the tension at the required point.

In submitting this formula to an experimental test, M. Kohlrausch made use of the wooden trough before alluded to. The copper and zinc plates were united, as in one of the experiments already described, by a long fine wire, bent from side to side of a wooden frame in a zigzag manner. The tensions of the points described below were determined by direct experiment. The electromotive force was also determined, the reduced length of the circuit was found by measuring the resistances of its various parts, and from these two, the electromotive force and the reduced length, the tensions due to the same points were calculated by the foregoing formula.

Points examined.

a. The second lower angle of the zigzag.

b. The fourth lower angle of the zigzag.

c. The sixth lower angle of the zigzag.

d. The point where the zigzag joined the copper.

e. The solution of sulphate of copper 2·02 inches from the plate of copper.

f. The solution of sulphate of copper 4·02 inches from the plate of copper.

g. The solution of the sulphate of copper 6 inches from the plate of copper.

h. The solution of sulphate of copper 8 inches from the plate of copper.

In the following table the results obtained by calculation are

compared with those obtained by direct experiment; the quantity λ is the same as that contained in the formula.

	λ.	*u* calculated	*u* observed
a	118·5	0·93	0·85
b	237	1·86	1·85
c	355·5	2·80	2·69
d	474	3·73	3·70
e	610 3	4·80	5·03
f	745·3	5·86	5·99
g	879	6·91	6·93
h	1014	7·98	7·96

The truth of Ohm's formula, which he derived from considerations purely theoretical, appears to be placed beyond the pale of doubt by these results. Hitherto the celebrated law which usually bears his name has rested upon hypothesis merely; and to the extraordinary patience and refined experimental skill of Kohlrausch is due the credit of giving to this conjectural foundation the stability of fact.

It may be stated, in addition, that the same physicist has also examined the thermo-circuit, and has not only demonstrated the existence of electric tension at its poles, but also proved that the electricity obeys the same law of distribution as that true for the voltaic circuit.

ON ELECTRO-MAGNETIC ATTRACTIONS.*

§ 1.

The subject of the present memoir is embraced by the following four propositions :—

I. *To determine the relation between the strength of an electro-magnet and the mutual attraction of the magnet and a mass of soft iron, when both are in contact.*

II. *To determine the same relation when the magnet and the iron are separated from each other by a fixed distance.*

III. *A constant force being applied to the iron in a direction opposed to the pull of the magnet, to determine the conditions of equilibrium between this force and magnetism when the distance between the magnet and the iron varies.*

IV. *To determine the relation between force and distance, that is to say, the law according to which the magnetic attraction decreases when the distance is increased.*

The first of these propositions has engaged the attention of physicists from time to time during a considerable number of years. Experiments have been made and facts multiplied, which, however, are so disunited and contradictory as to render any attempt to reduce them to a common law altogether hopeless.

The most important experiments which have been made in connection with this subject are those of Lenz and Jacobi; who established, that the attraction between two electro-magnets, or an electro-magnet and a mass of soft iron, is proportional to the square of the magnetising current. Here, however, an interval of about $\frac{1}{10}$th of an inch separated the attracted mass from the magnet,—a condition the importance of which appears to have been overlooked by the discoverers of the law. It has been generally assumed, that if the defects incidental to

* Phil. Mag. April, 1851.

the modes of experiment hitherto adopted could be avoided, the same law would pronounce itself in the case of contact. Were this the case, our two first propositions would be identical, the solution of the one would necessarily imply the solution of the other; it will be shown, however, that the laws of action in both cases are entirely different.

Two principal causes have been assigned as giving birth to the discrepancies alluded to—the incompleteness of contact, and the peculiar shape of the mass of iron attracted; the question naturally occurs, cannot these causes of divergence be removed?

1. To annul, as far as possible, the disturbances arising from mere form, a number of regularly-shaped pieces of good soft iron were procured; they included cubes, cylinders, and spheres of various diameters. It is easy to see the practical difficulty of experimenting with cubes and cylinders; indeed, to render such experiments pure, conditions are required which it is almost impossible to fulfil. Conceive the cube suspended by a wire attached to the middle of one of its faces, and laid with its opposite face flat upon the polished end of the magnet; let the wire ascend vertically, pass over a pulley, and be attached to a scale-pan at the other side; on this scale-pan let weights be laid until the cube is separated from the magnet; the weight which effects the separation expresses the sustaining power. That the experiment, however, shall be faultless, it is necessary that all parts of the surface of the cube should give way at the same moment, otherwise the cube will hold on by its edges and corners, and thus totally vitiate the experiment. To effect this, it would be necessary, first, that the production of the wire should go exactly through the centre of gravity of the cube; and secondly, that all portions of the surface should be equally in contact, or that any deviation from the one condition should be compensated by a deviation from the other. The difficulty of complying with these requirements has compelled me to abandon both cubes and cylinders, and to resort to a body with which the smallness of the surface of contact reduces the irregularity to a minimum; a body, moreover, which is able to accommodate itself to the slight divergences of the wire. That body is the sphere.

2. The magnet used was that formerly applied by my friend Professor Knoblauch and myself in an investigation 'On the

Magneto-optic Properties of Crystals.'* As then used, it consisted of two soft iron cylinders set upright in a glass case, and united below by a cross-piece of iron. Round the cylinders were coiled 360 feet of copper wire, weighing nine pounds, and upon the top of the cylinders two finely polished parallelopipeds of iron were laid, between which the crystal was suspended. In the present case the cross-piece was removed, and the two cylinders were tightly screwed together—an arrangement provided for in their construction—and thus converted into a single magnet, 9 inches long, 1·3 inch thick, surrounded by a helix containing 360 feet of copper wire. The magnet was made fast in a block of wood, and set vertically upright under one end of the beam of a fine balance; from this end the ball of iron was suspended by a copper wire; the length of the latter was so arranged, that, when stretched full, the ball resting on the end of the magnet, a whalebone index pointing to the zero of a graduated arc showed that the balance-beam was horizontal. From the other end of the beam a scale-pan was suspended which held the weights. The weight of the ball and its attached wire was, in the first place, exactly balanced by a counter-weight; so that when the magnetism was excited, the attractive force measured by the other weights was a purely magnetic force.

After a few experiments a slight modification of the above arrangement was found necessary. The end of the iron core on which the ball rested had a little cavity in its centre, which resulted from its having been turned in a lathe. It being absolutely necessary that the ball should rest exactly upon the centre, a parallelopiped of soft iron was placed upon the end of the magnet. The two diagonals were drawn upon one of its polished horizontal faces, and the iron sphere always rested upon the point of intersection.

3. During the investigation, the battery, the magnet, and the instrument used to measure the intensity of the current (Weber's tangent galvanometer), were in three different rooms. From the poles of the battery two long bands of sheet copper ran side by side, and passed thus under the door into the room which contained the magnet. One of them was carried to the magnet,

* Phil. Mag. March, 1850, and July, 1850.

and the other to the tangent galvanometer, which was also connected by a similar band with the magnet.

The copper ring of the tangent compass, for which I am indebted to the kindness of Professor Bunsen, is 16 inches inner diameter, the diameter of the needle-box is 7 inches, and the limb is graduated to intervals of 10′. The needle is short, and carries at one end a straight fibre of glass, as fine as a hair, which points to the graduated circle. To avoid parallax, the fibre was reflected from a metallic surface underneath; in reading off, the fibre covered its image. The needle comes quickly to rest; and by means of a small eye-glass, the angle can be read to 5′ with the greatest ease.

4. We have here the means of exciting the magnet and of measuring the power of the exciting current, but not yet the means of varying the latter. This was effected by a *rheostat*, which was placed upon the same bench with the magnet. The instrument consisted of a stone cylinder capable of being turned by a handle. Round the cylinder a thin wire was coiled, which offered a powerful resistance to the passage of the current. By turning the handle, any required length of this wire could be thrown into the circuit, and the strength of the current thus varied at pleasure.

5. The following is the method which was first pursued, being in substance the same as that usually adopted in cases of this nature:—The iron ball being balanced in the manner before described, a current was sent round the magnet; the ball was attracted, and came to rest on the centre of the surface presented to it. Weights were then laid upon the opposite scale-pan until the ball was torn away. Although every precaution was taken to secure accuracy, the results thus obtained were not so satisfactory as might be desired; for even when the current remained constant, the weight necessary to separate the ball varied considerably in different experiments. This was also the case when fine shot was poured upon the scale-pan. In the laying on of weights or the pouring in of shot, it is scarcely possible to avoid a vibration of the beam, which communicates itself to the ball in a series of slight jerks directed upwards. The weight upon the opposite scale-pan is ever ready to take advantage of the slightest loosening of the ball occasioned by these little jerks, and hence a sepa-

ration may take place which is not due to a fair overpowering of the magnet by gravity.

6. The thought occurred to me that the cause of disturbance might be removed by using a variable magnet and fixed weight instead of a fixed magnet and a variable weight. Hitherto I had excited the magnet by a certain current, and added weights until the ball gave way. The method which now suggested itself was to lay a certain weight upon the scale-pan, and slowly to reduce the magnetic power until a separation took place. By means of the rheostat I had this completely in my power, and a few trials sufficed to demonstrate the superiority of this mode of experiment over the former.

The method of proceeding being thus determined, a friend in whose skill and fidelity I could rely* took charge of the reading of the tangent galvanometer. I took up my place before the magnet, weighted the balance, turned the rheostat, and observed the moment of separation; at this moment the turning of the rheostat ceased, and the deflection was noted.

§ 2.

7. Proposition I.—*To determine the relation between the strength of an electro-magnet and the mutual attraction of the magnet and a mass of soft iron, when both are in contact.*

It is necessary here to define what is meant by 'the strength of an electro-magnet.'

A magnetised needle placed perpendicular to the magnetic meridian is solicited *towards* that meridian by the earth's magnetism with a certain force, H.

A magnetised needle, placed parallel to the magnetic meridian, is solicited *from* it by a distant magnet, set perpendicular to the meridian, and so that the axis of the magnet produced strikes the centre of the needle, by a certain force, h.

When the earth and the magnet act *together* upon the needle, it will take up a position oblique to the magnetic meridian. Let the angle which the needle makes with the latter be a.

The effective portion of H which now tends to *turn* the needle is

$$H \sin a.$$

* Mr. Thomas Hirst.

The portion of h which tends to turn the needle in the opposite direction is

$$h \cos a.$$

These forces are now in equilibrium, and hence

$$\text{H} \sin a = h \cos a,$$

or

$$h = \text{H} \tan a.$$

Supposing the power of the magnet to be changed, that it has become stronger or weaker, without, however, having its position changed. Let h' be the force corresponding to h in the former case, and a' the angle corresponding to a, then we have

$$h' = \text{H} \tan a',$$

and hence

$$\frac{h}{h'} = \frac{\tan a^{*}}{\tan a'}.$$

The quantities h and h' express the relative strengths of the magnets, these strengths being proportional to the tangent of the angle which the needle makes with the magnetic meridian.

8. If the magnet be an electro-magnet, we have the means of measuring the intensity of the current which circulates in the surrounding helix. It is proportional to the tangent of the angle (β) which the needle of the tangent galvanometer, under the influence of the current, makes with the magnetic meridian.

9. Lenz and Jacobi have proved, that for a double intensity tangent a is doubled, for a treble intensity it is trebled; in general, that the strength of the magnet is directly proportional to the intensity of the exciting current.† But the latter, as we have said, is proportional to tangent β; and hence, in the following investigation, *tan β is assumed to express 'the strength of the magnet.'*

M. Müller of Freiburg denies the universality of this law. But a reference to the dimensions of our magnet will show, that,

* The same demonstration holds good for the tangent galvanometer, if instead of the magnet in its different states we substitute currents of different powers. The tendency of the latter being to set the needle at right angles to the magnetic meridian, their action is precisely similar to that of the magnet.

† Poggendorff's *Annalen*, vol. xlvii. p. 244.

even supposing M. Müller's objections to be well-grounded, they are not in the slightest degree applicable at present. In all cases the tangent of the aforesaid angle may be taken as the accurate expression of the magnetic force.

10. Table I. contains the results obtained during one of the earliest trials of the new method of experiment. After balancing the ball, a weight of 300 grammes was laid upon the scale-pan, the rheostat being so arranged that very little wire was in the circuit. The magnet was excited and the ball attracted. By turning the handle of the rheostat, and thus slowly damping the current, a point was at length attained where the ball gave way; here the turning of the handle ceased, and the angle of deflection was read off. The rheostat was then turned backwards to its former position, a weight of 10 grammes was then added to the 300 and the same process repeated. A series of equivalents for the magnetic attraction was thus found, increasing by a common difference 10. The angles and the weights corresponding to them are set side by side in the following table, the former being expressed in degrees and decimals of a degree.

11. To ascertain the ratio of the weight to the corresponding magnetic force, the former ought, strictly speaking, to be divided by tan β; but for the angles which here appear, the tangents are proportional to the arcs, and hence the latter are, in the present instance, made use of as divisors. In all other tables throughout this memoir the angles are expressed in degrees and minutes; and the tangents of the angles, instead of the arcs themselves, are made use of.

Table I.

β.	W.	$\frac{W}{\beta}$.
°	grms.	
8·5	300	35·3
8·8	310	35·2
9·12	320	35·3
9·5	330	34·8
9·64	340	35·3
10·00	350	35·0
10·25	360	35·1
10·5	370	35 0
10·75	380	35·3
11·0	390	35·4
11·25	400	35·5
11·5	410	35·6
11·62	420	35·8
12·0	430	35·8

The weights here applied vary only within narrow limits. Another series of experiments, commencing with 300 grammes and ending with 900, follows. An addition of 100 grammes was made at every new determination. For the sake of convenience, the weight is made the divisor; and to avoid decimals, tan β is multiplied by $10{,}000 = q$.

TABLE II.

W.	β.	$\frac{q \tan \beta}{W}$.
grms.	° ′	
300	6 35	3·83
400	9 15	4·07
500	11 30	4·06
600	13 30	4·00
700	16 0	4·10
800	17 53	4·04
900	20 0	4·04

Another series, in which 150 grammes were added after each experiment, is here introduced.

W.	β.	$\frac{q \tan \beta}{W}$.
grms.	° ′	
300	7 0	4·10
450	10 15	4·03
600	13 40	4·05
750	17 25	4·19
900	20 10	4·08

In Table I. we observe that $\frac{W}{\beta}$ is a constant quantity, and in the other tables we see that $\frac{\tan \beta}{W}$ is constant. This leads us to the following law:—*The mutual attraction of an electro-magnet and the sphere of soft iron, when both are in contact, is directly proportional to the strength of the magnet.*

12. The helix which surrounded the core used by Lenz and Jacobi was composed of two separate wires. The two ends of one of these wires were connected with the poles of the battery, while the ends of the other were connected with a galvanometer. On sending a current through the former, a secondary current was induced in the latter, which ran round their galvanometer, imparting in its passage a shock to the magnetic needle. Calling the extreme angle attained by the needle in consequence of this shock ϕ, it is easy to prove that

the induced current is proportional to $\sin\frac{1}{2}\phi$. But the magnetism of the core is known to be proportional to the induced current which it is able to excite,* and hence we have the strength of the magnet proportional to $\sin\frac{1}{2}\phi$. The magnet with which I experimented was not furnished with two wires such as those described; but a second helix happened to be at hand, into which an iron core could be introduced. This helix and its core were placed upon the end of the magnet, the axis of the core being a continuation of the axis of the magnet. On exciting the latter the core was also excited, and thus a current was induced in the wire of the surrounding helix, which was connected with a distant astatic galvanometer. A glass containing mercury was introduced into the voltaic circuit near the magnet; two stout copper wires connected with the bands of sheet copper before-mentioned dipped into the glass; one of these could be suddenly lifted out of the mercury. To this one a string was attached, which passed over a pulley placed above it and thence to the distant galvanometer. By pulling the string, the wire left the mercury and the circuit was broken; by letting the string go, the wire fell into the mercury again and the circuit was closed.

13. In this way, not only the indications of the tangent galvanometer, but also those of the astatic galvanometer were observed for each weight. The strength of the magnet being, as before observed, proportional to $\sin\frac{1}{2}\phi$, the quotient $\dfrac{\sin\frac{1}{2}\phi}{W}$ ought to be a constant quantity, and hence

$$\frac{\tan\beta}{W} = \frac{n\sin\frac{1}{2}\phi}{W},$$

where n is a constant which expresses the ratio of $\tan\beta$ to $\sin\frac{1}{2}\phi$.

In the following series n is equal to 0·71; both quotients are multiplied by q for the reason before assigned.

* Poggendorff's *Annalen*, vol. xlvii. p. 230.

TABLE III.

W.	β.	$\frac{1}{2}\phi$.	$\frac{q \tan \beta}{W}$.	$\frac{0{\cdot}71\, q \sin \frac{1}{2}\phi}{W}$.
grms.	° ′	° ′		
300	8 15	11 45	4·83	4·83
400	11 10	15 30	4·92	4·90
500	13 25	19 45	4·78	4·80
600	15 45	23 30	4·70	4·72
700	18 0	27 30	4·64	4·68
800	20 0	32 0	4·55	4·69

The constancy of the quotient in the fifth column is a corroboration of the former experiments.*

§ 3.

14. The order of the propositions at the commencement is that which I thought best calculated to convey an idea of the nature of the investigation. It will be shown, however, that the third and fourth propositions once solved, the second may be derived from them as a corollary; I shall therefore pass on to the consideration of—

PROPOSITION III.—*A constant force being applied to a mass of soft iron in a direction opposed to the pull of the magnet, to determine the conditions of equilibrium between this force and magnetism when the distance between the magnet and the iron varies.*

15. A sheet of foreign post paper, $\frac{1}{1000}$th of an inch in thickness, was cut into a number of small squares. One of these squares was laid upon the flat pole; the magnet was excited, and the iron ball brought down upon it. A weight of 100 grammes was placed upon the opposite scale-pan, the handle of the rheostat was slowly turned till the ball gave way, and the angle of the tangent galvanometer was then read off. A second leaf was laid upon the former, the rheostat was brought back to its original position, the ball was brought down upon the paper, and exactly the same process was repeated. The weight on the scale-pan remained constant during the entire series of experiments, viz. 100 grammes. Now it is easy to see that the greater the distance which separates the ball from the magnet, the greater must the power of the latter be to counter-

* The method of proving the strength of a magnet before the year 1780, when Coulomb published his researches, which method consisted simply in ascertaining how much the magnet was able to lift, appears from the above to be essentially correct.

balance the force acting against it. We have thus the equation

$$m=f(d),$$

where m represents the magnetism of the core, and d the distance which separates ball and magnet. It will be proved immediately that this function possesses the form

$$m=n\sqrt{d},$$

where n is a constant. This expressed in words announces the remarkable law, that *when the distance between the magnet and the sphere of soft iron varies, and a constant force opposed to the pull of the magnet is applied to the latter, to hold the ball in equilibrium the power of the magnet must vary as the square root of the distance.*

The quantity m is expressed by tan β. The proof of the above law will therefore depend on the fulfilment of the equation

$$\frac{\tan\beta}{\sqrt{d}}=\text{const.}$$

In the following table the thickness of a leaf is taken as the unit of distance, and tan β is multiplied by $100=r$.

TABLE IV.

Number of leaves.	β.	$\frac{r\tan\beta}{\sqrt{d}}$.
	° ′	
1	6 10	10·9
2	8 45	10·9
3	10 45	10·9
4	12 0	10·6
5	13 20	10·6
6	14 25	10·5
7	15 50	10·7
8	16 40	10·6
9	17 45	10·7
10	18 30	10·6

In the following series the experiments are continued to greater distances. A constant weight of 200 grammes was here placed upon the scale-pan.

Number of leaves.	β.	$\frac{r \tan \beta}{\sqrt{d}}$.
	° ′	
11	23 45	13·2
12	24 45	13·3
13	25 15	13·1
14	26 5	13·1
15	27 10	13·2
16	27 50	13·2
17	28 35	13·2
18	29 0	13·1
19	29 50	13·1
20	30 15	13·0
21	31 0	13·3
22	31 40	13·2
23	32 25	13·2
24	33 10	13·3

We here observe that the condition $\frac{\tan \beta}{\sqrt{d}} = \text{const.}$ is satisfied to a remarkable degree of exactitude.

16. Considering the electro-magnet in its separate states, during the above experiments, to represent a number of distinct magnets of different strengths, we see that if one magnet have twice the strength of another, and if the latter exercise a certain force at a certain distance, the former will exercise an equal force at four times this distance; if one magnet have three times the strength of the other, the former will exercise an equal force at nine times the distance; and so on.

§ 4.

17. An apparently well-grounded doubt, however, may attach itself to all these experiments. In damping the current, we descended from a point at which the full power was exerted to that at which the ball gave way. Now it is well known that a soft iron core, when once excited, does not instantly lose its magnetism on the cessation of the current, but continues active for a short time afterwards. The question, 'may not the magnetism *lag* in the core during the descent above alluded to?' naturally suggests itself here. But if the decrease of the current be not immediately accompanied by a proportionate fall of the magnetic power, the strength of the magnet at the moment the ball separated is not the true strength due to the current *then*

present, but a strength due to a greater current. It will be seen immediately that experiment seems to justify this doubt.

18. To avoid everything of this kind, it was only necessary to ascertain pretty nearly beforehand at what particular point in the wire of the rheostat the ball gave way; and then, instead of turning the handle backwards until all the wire was excluded, to turn it back so far as to permit the passage of a current which was barely sufficient to overcome the weight upon the scale-pan. The descent from this point to that where the ball separated was so very short, that the fraction of magnetism remaining, if such remained at all, might be neglected. Applying this mode of experiment, the case of contact was first investigated with three smooth spheres of the following dimensions :—

No. 1,	diameter 0·95 of an inch,		weight 65·25 grms.	
No. 2,	...	0·48 ...	... 9	...
No. 3,	...	0·287 ...	... 1·7	...

The arrangement of the following table is similar to that of Table II. In each case the angle given is the mean of four observations.

Table V.

No. 1			No. 2			No. 3		
W	β	$\frac{q \tan \beta}{W}$	W	β	$\frac{q \tan \beta}{W}$	W	β	$\frac{q \tan \beta}{W}$
grms.	° ′		grms.	° ′		grms.	° ′	
250	7 1	4·92	100	7 40	13·5	30	7 24	4·33
275	7 25	4·72	125	9 27	13·3	40	9 36	4·22
300	8 1	4·70	150	11 10	13·2	50	10 53	3·84
325	8 38	4·67	200	14 0	12·4	60	12 31	3·70
350	9 7	4·57	225	15 1	11·9	70	14 30	3·70
375	9 43	4·56	250	16 30	11·8	80	16 1	3·58
400	10 15	4·52	275	18 2	11·8	90	17 28	3·50
425	10 55	4·54	300	19 3	11·8	100	18 26	3·33
450	11 27	4·51	325	20 20	11·4	110	19 53	3·26
475	11 47	4·40	350	21 21	11·2	120	21 27	3·27
500	12 17	4 36	375	22 15	10·9	130	22 42	3·21
525	12 53	4·36	400	23 17	10·7	140	23 37	3·12
550	13 22	4·33	425	24 40	10·8	150	25 0	3·10
575	13 53	4·29	450	25 41	10·6	160	26 7	3·06
600	14 16	4·23	475	26 36	10·5	170	27 3	3·00
650	15 18	4·21	500	27 41	10·5	180	28 58	3·07
700	16 10	4·14	525	28 37	10·4	190	29 41	3·00
750	17 6	4·10	550	29 48	10·4	200	31 48	3·10
800	17 57	4·05	575	30 27	10·2			
850	18 48	4·03	600	31 33	10·2			
900	19 48	4·00						

19. In all these cases we observe a gradual decrease of the quotient $\frac{\tan \beta}{W}$ from top to bottom. The same discrepancy exhibits itself when we operate with the leaves. This is shown by the following table, the arrangement of which is similar to that of Table IV.

TABLE VI.

	Ball No. 1. Weight 100 grms.		Ball No. 2. Weight 20 grms.		Ball No. 3. Weight 10 grms.	
Number of leaves	β	$\frac{r \tan \beta}{\sqrt{d}}$	β	$\frac{r \tan \beta}{\sqrt{d}}$	β	$\frac{r \tan \beta}{\sqrt{d}}$
	° ′		° ′		° ′	
1	—	—	—	—	10 40	18·8
2	—	—	—	—	14 26	18·1
3	11 28	11·7	11 17	11·5	17 5	17·7
4	12 59	11·5	12 45	11·3	19 8	17·3
5	14 0	11·1	13 51	11·0	20 57	17·1
6	14 52	10·8	15 6	11·0	22 51	17·2
7	15 46	10·7	16 10	10·8	24 22	17·1
8	16 41	10·6	17 2	10·8	25 51	17·1
9	17 16	10·4	17 56	10·8		
10	18 0	10·3	18 32	10·6		
11	18 41	10·2	19 21	10·5		
12	19 12	10·0	20 12	10·6		
13	19 57	10·0	20 52	10·5		
14	20 17	9·9	21 22	10·4		
15	20 47	9·8	22 0	10·4		
16	21 22	9·8	22 42	10·4		
17	21 41	9·6	23 17	10·4		
18	22 1	9·5	23 55	10·4		
19	22 34	9·5	24 34	10·4		
20	23 0	9·5	25 1	10·4		
21	23 22	9·4	25 40	10·4		
22	23 44	9·4	26 5	10·4		
23	24 10	9·3	26 31	10·4		
24	24 39	9·3	27 1	10·4		
25	24 52	9·3	27 25	10·4		
26	25 32	9·4				
27	25 57	9·4				
28	26 15	9·3				
29	26 34	9·3				
30	26 55	9·3				

20. The deviations here exhibited are such as might be supposed to occur, were the doubt mentioned at the commencement of this section well-grounded. For when the weights are small, as at the beginning of the columns, if we commence at the top of the rheostat a long descent is necessary before the ball yields; and it seems reasonable to infer, that, the longer the descent, the greater would be the amount of *lagging magnetism* present in the core at the moment of separation. Two combined causes would thus operate to hold the ball; the mag-

netism due to the current, and that which lingers behind the current. It is evident that were the latter removed, to hold the ball we must increase the former. Any mode of experiment, therefore, which does away with this lingering portion of the force of the magnet ought to give us greater deflections at the tangent-galvanometer than a method which permits of its exhibition, the increase being most appreciable where the weights are small. This our last mode of experiment actually does. Were the angles at the top of the columns in the last two tables a little smaller, the results would be the same as those in Tables I., II. and III. It seems, therefore, at first sight fair to attribute the extraordinary agreement among the earlier results, in part, to lagging magnetism, and hence to infer that the law is not so precise and simple as they would indicate.

21. Plausible as this appears, I cannot accept it as the true explanation of the above divergencies. If they be due to lagging magnetism, it is only necessary to give the magnet time to subside to cause this to disappear. The old method of experiment was again resorted to. A weight of 500 grammes was laid upon the scale-pan, and, commencing on the top of the rheostat, the handle was turned and the current damped until the ball gave way. This was found to occur as the handle of the instrument made its thirty-seventh revolution. A series of four descents were first made, slowly, but without pausing; then four more, stopping for two minutes at the thirty-fifth revolution, and thus allowing the lagging magnetism, if such existed, to subside. Supposing such to exist, then the deflections exhibited in the last four experiments must be greater than those exhibited in the first four; for the subsidence of the lagging magnetism must be compensated by an increase of current, the said increase being indicated by a greater angle. This conclusion, which necessarily flows from the above assumption, is, however, diametrically opposed to fact. The angles in the last four experiments, instead of being greater, are actually less than the others. They are as follows:—

Weight 500 grammes.

	Without pausing.	Pausing 2′ at the 35th revolution.
1	11° 25′	11° 10′
2	11 15	11 10
3	11 25	11 0
4	11 10	11 5

It is thus proved that the deviations cannot be accounted for on the supposition that the magnetism lingers in the core after the current has fallen. Further on an attempt will be made to exhibit their true origin.

22. From Table V. we derive a notion of the influence of *size* upon the attraction of an iron sphere. By selecting the weights common to all three balls, and comparing the corresponding magnetic powers, we learn, that, to support the same weight, the ball No. 2, when in contact, requires a current 2·4 times as strong as that required by No. 1, and that No. 3 requires a current 2·4 times that required by No. 2. In the following table this multiplication by the factor 2·4 is carried out for the two first balls. The first three figures of the tangents have been taken; and as we have simply to do with ratios, and not with absolute values, these three figures are treated as whole numbers.

Ball No. 1. W. grms.	tan β.			Ball No. 2. tan β.
250	123	× 2·4 =	295	296
275	130	...	312	326
300	141	...	338	346
325	152	...	365	371
350	160	...	384	391
375	171	...	410	409
400	181	...	420	430
425	193	...	463	459
450	203	...	487	481
475	209	...	501	501
500	218	...	523	525
525	229	...	549	546
550	238	...	571	573
575	247	...	593	588
600	254	...	609	614

§ 5.

23. Magnetic attraction, like the attraction of gravitation, is the result of a reciprocated force. The attraction of a sphere of soft iron depends, not only upon the magnetism of the magnet, but also upon that of the excited ball. The attraction is equal to the product of both. In operating with balls of different diameters, two things are to be taken into account, which, in default of better terms, may for the present be called *quantity* and *intensity*; the former depending upon the volume of the

ball, though not proportional to it; the latter on the power of the magnet, to which it is proportional. According to this view, the intensity of magnetism in a ball of a certain diameter, placed at a certain distance from the magnet, is the same as that of a ball of twice or half the diameter placed at the same distance; but the quantities of magnetism are very different. The attraction of the ball depends upon both quantity and intensity. Thus the force with which the sphere reciprocates that of the magnet may be regarded as being made up of the two components q and i, the former of which stands for the quantity, the latter for the intensity. Let m be the magnetism of the magnet, then the attraction of a sphere placed *at a small distance* * will be expressed by the product

$$mqi.$$

It has been already proved, that to hold a certain weight in equilibrium, the ball No. 2, when in contact, requires 2·4 times the magnetic power that No. 1 requires. But I found this to vary a little when small intervals existed between the ball and magnet. When a film of mica $\frac{1}{5000}$th of an inch in thickness was placed upon the pole, the above factor reduced itself to 2·3; at a leaf thickness, or $\frac{1}{1000}$th of an inch distant, it was 2·25; at $\frac{1}{250}$th of an inch distant it was 2·22; while at $\frac{1}{30}$th of an inch distance it was nearly the same as in contact. For our present purpose, the number 2·25 is nearest the truth. At any given moment let m_0 be the magnetism of the magnet, and q_0 and i_0 the magnetic components of the ball No. 1; then, as before observed, the attraction of the ball will be

$$m_0 q_0 i_0.$$

Let m_1, q_1 and i_1 represent the corresponding quantities for the ball No. 2, its attraction will be

$$m_1 q_1 i_1.$$

But each of these attractions is measured by the weight which it can hold in equilibrium; and if the same weight be used in both cases, we shall have

$$m_1 q_1 i_1 = m_0 q_0 i_0. \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (1)$$

* The importance of this condition will appear further on.

But for the same weight, remembering that the stronger magnetism is applied to the smaller ball, we have

$$m_1 = 2{\cdot}25 m_0\,;$$

and as the intensity is assumed proportional to the magnetism of the magnet, we have also

$$i_1 = 2{\cdot}25 i_0.$$

Substituting these values in equation (1), we obtain

$$5 m_0 q_1 i_0 = m_0 q_0 i_0,$$

or

$$q_0 = 5 q_1.$$

As the quantity depends solely upon size, the relative quantities of two balls may be supposed to remain constant, whatever be the power of the magnet. When, therefore, the same magnetic power is applied to both balls, the difference between the forces exerted upon them will depend solely on the factors q_0 and q_1; for in this case we should have $m_0 = m_1$ and $i_0 = i_1$. But it has been proved that $q_0 = 5q_1$; hence, with the same magnetic power, the attraction of the large ball ought to be *five times* that of the small one. A remarkable coincidence with this deduction is exhibited in Table VI. At the top of the columns the magnetic forces applied to both balls happen to be nearly alike, and the attraction of No. 1 is in those cases precisely *five times* that of No. 2. The same coincidence is exhibited in the following table :—

Ball No. 1.		Ball No. 2.	
W.	tan β.	tan β.	W.
grms.			grms.
50	0·141	0·138	10
100	0·203	0·197	20
150	0·248	0·240	30
200	0·287	0·280	40
250	0·322	0·314	50
300	0·349	0.344	60
350	0·377	0·372	70
400	0·399	0·396	80
450	0·424	0·420	90

During the latter experiments each ball was separated by a fixed distance of $\frac{1}{250}$th of an inch from the pole. We see that the corresponding magnetic powers expressed by tan β are

nearly equal throughout, but that the corresponding weights are in the constant ratio of 5 : 1.

§ 6.

Proposition IV.—*To determine the relation between force and distance; that is to say, the law according to which the magnetic attraction decreases when the distance is increased.**

24. There was some little difficulty in applying our mode of experiment in the present case. Did we resort to the method of laying on weights or of pouring in shot, nothing could be apparently easier than to determine this law. We should simply have to preserve the magnetic power constant; to place, by means of the interposed leaves, various intervals between the sphere and magnet, and to determine in each case the weight necessary to break the hold. The objections to this method have, however, been already stated. The plan of proceeding will perhaps be rendered plainer by reference to a method sometimes adopted to determine the melting-point of wax or fatty matter. A little glass tube containing a portion of the matter is dipped into water of a known temperature—it does not melt. It is now dipped into water of a higher temperature—it melts. It is evident that the melting-point lies somewhere between these two temperatures, and that by approximating the temperatures of the fluids, the exact melting-point can at length be obtained. Our proceeding with the magnet was precisely similar. A magnetic power due to a current of 25° was taken as a standard. A certain weight was laid upon the scale-pan, and a current somewhat greater than 25° was sent round the magnet. The ball was brought down upon the leaf or leaves which measured the distance from the pole, and the rheostat was slowly turned until the ball gave way. If this occurred at 25°, the right weight for the distance in question had been chosen. Suppose, however, that the ball preserved its hold till the current was damped to 23°; it is plain that the weight chosen was too small, and a little

* Some little difficulty may be encountered in the attempt to separate the third proposition from the fourth. This will vanish when it is considered, that in the former case a constant force (a weight) operated against the magnet, and the question was one between *magnetism* and *distance*; in the latter case, the magnetism is preserved *constant*, and the question is one between *weight* and *distance*.

must be added. If the ball, with this addition to the weight, yielded before it reached 25°, then the weight added was too large. Thus by a method of approximation similar to that above described, the exact weight due to a certain distance and to a magnetic power of 25° was obtained. From the description this will appear a circuitous process, but it is not so practically. With a little experience the proper weight can be ascertained with great despatch.

25. The following table contains a series of experiments made in this manner with ball No. 2:—

TABLE VII.

Strength of current 25°. Paper $\frac{1}{1000}$th of an inch thick. Ball No. 2.

No. of leaves.	Weight supported. grms.	W × *d*. grms.
2	150	300
3	110	333
4	87	348
5	75	375
7	56	392
8	50	400
9	45	405
10	40	400
11	37	407
12	34	408
13	31½	409
14	29¼	409
15	27	405
16	25½	408
17	24	408
18	23	414
19	21½	408
20	20¼	405
21	19¼	406
25	16¼	406
30	13½	405

We here see, that, after a distance of $\frac{1}{125}$th of an inch from the pole has been attained, the product of the weight and distance is constant. The divergencies in the immediate neighbourhood of the pole belong to the class the discussion of which will be resumed further on. A slight correction will cause them to vanish. Hence we have arrived at the law, that *the attractive force between the magnet and sphere varies inversely as the distance.*

26. M. Dub, of Berlin, has lately produced an elaborate

memoir on the attraction of electro-magnets.* Though he has operated with masses of iron of very divergent shapes, the above law is found, on closer examination, to pronounce itself with more or less distinctness throughout the whole of his experiments. I will take the liberty of copying his first table.

Attraction of iron bars (submagnets) 6 inches long and 1″, ¾″, ½″ and ⅜″ thick, by a magnet 12 long and 1″ thick, and a strength of current of 20°.

Contact.	Submag. 1″ thick. lbs.	Submag. ¾″. lbs.	Submag. ½″. lbs.	Submag. ⅜″. lbs.
	3·27	4·1	4·76	5·1
¼	1·1	1·25	1·4	1·6
½	0·9	0·9	0·92	0·95
¾	0·71	0·77	0·65	0·65
1	0·6	0·65	0·48	0·45
2	0·38	0·36	0·23	0·194
3	0·27	0·23	0·15	0·11
4	0·19	0·16	0·11	0·08
5	0·15	0·12	0·08	0·062
6	0·11	0·1	0·07	0·05
7	0·095	0·08	0·062	0·044
8	0·08	0·063	0·05	
9	0·07	0·055	0·04	

The numbers in the first column denote the distance between the bar and the magnet, and the numbers in the columns opposite express, in pounds, the attraction of the respective bars. The distance was regulated by means of a screw, a quarter of a revolution being denoted by the fraction ¼. The numbers 1, 2, 3, &c., express 1, 2, 3, &c., revolutions. One revolution corresponded to $\frac{1}{45}$th of an inch distance; the experiments therefore commenced when a distance of $\frac{1}{180}$th of an inch separated the magnet from the bar of iron.

Multiplying the weights in each column by their corresponding distances, we obtain the following result:—

Distance.	Subm. No. 1.	Subm. No. 2.	Subm. No. 3.	Subm. No. 4.
¼	0·27	0·31	0·35	0·4
½	0·45	0·45	0·46	0·47
¾	0·53	0·57	0·49	0·49
1	0·60	0·65	0·48	0·45
2	0·76	0·72	0·46	0·38
3	0·81	0·69	0·45	0·33
4	0·76	0·64	0·44	0·32
5	0·75	0·60	0·42	0·31
6	0·66	0·60	0·42	0·30
7	0·66	0·56	0·40	0·31
8	0·64	0·50	0·40	
9	0·63	0·50		

* Poggendorff's *Annalen*, vol. lxxx and lxxxi.

When we consider the disturbing action of the edges and the difficulty of preserving perfect parallelism between magnet and submagnet, we shall not be surprised at the deviations which exhibit themselves close to the poles. At a little distance the law of action expressed above receives a good corroboration.

A series of experiments was made in 1841 by M. Cramer, mechanician to the university in Kiel, with a view to determine the influence of distance upon the lifting power of the magnet. He experimented with steel magnets of the horse-shoe form, placing, in each case, the unlike poles of two magnets of the same size exactly opposite to each other. The distances between the poles were measured, as in our case, by leaves of paper. The result of M. Cramer's investigation, as contained in vol. lii. p. 302 of Poggendorff's *Annalen*, is as follows :—

'Within the limits of these experiments the magnetic attraction by no means decreases in the inverse ratio of the square of the distance (*sehr natürlich*, P.).

'When the various columns are compared, there is so little agreement exhibited, and even the members of the same series decrease so irregularly, that it appears impossible to refer them to any definite law. This lawlessness is to me as unexpected as it is enigmatical. I cannot attribute it to the inaccuracy of the method pursued, which in itself is simple, and in the carrying out of which I expended all possible care and pains.'

The irregularity of decrease mentioned by the author appears to arise from his manner of viewing the subject. He begins his experiments by placing one leaf between the poles, and determining the weight necessary to separate the magnets. A second leaf is then introduced, and the separating weight again determined. The experiments are continued until a distance of 46 leaves is attained. The author then subtracts each weight from the next preceding, and in the column of numbers thus obtained he observes the irregularity complained of. M. Cramer's experiments, however, seem to prove decisively, that the law above developed for a sphere of soft iron and a straight core, magnetised by induction, is also true for two steel magnets of the horse-shoe form. Commencing at the 5th leaf in Table II., and multiplying the distances by the

weights corresponding to them, we obtain the following series of numbers :—

5	270	10	280	16	276	21	281
6	270	11	275	17	270	22	283
7	280	12	276	18	274	23	284
8	292	13	268	19	278	—	—
9	279	15	273	20	280	46	299

The products here may be regarded as constant, as the slight and irregular differences which they exhibit are evidently due to the mode of experiment.

§ 7.

27. Bodies capable of magnetisation are divided into two classes, in one of which the magnetic force is readily aroused, but as quickly lost again when the exciting cause is removed; the other class, on the contrary, accepts the magnetic state with difficulty, but retains it when once excited. Soft iron is an example of the one class, hard steel is an example of the other. When a piece of soft iron is acted upon by a magnet, it is said to be magnetised by influence. The magnet in this case is, so to speak, the creator of the force which responds to its attraction. At the commencement of this inquiry we arrived at the notion of the strength of a magnet from its action upon a freely suspended magnetic needle. The motion of the needle is the result of reciprocal action; but the force with which the needle reacts upon the magnet is not the gift of the latter, as in the case of soft iron. Adhering to the common notion of a magnetic fluid, in the case of soft iron the magnet decomposes and disposes of this fluid so as to cause mutual attraction. In the needle, the fluid is, so to speak, fitted by previous treatment for the action of the magnet. The steel in the case before us resists magnetisation by influence; and the magnet acts upon an independent fluid, without either increasing or diminishing the quantity thereof. Were this otherwise, tan a (7) would not be a proper measure of magnetic power. Let us suppose the case of a little ball of steel thus independently magnetised; and, to make the matter easier, we will suppose it charged with one of the fluids only, say the north fluid. Let the force with which the south pole of a magnet attracts the ball when placed at the unit of distance be a; then, according to the common law of magnetic

attraction (where the pole is supposed to be contracted to a point), the force of attraction at any other distance, r, would be $\frac{a}{r^2}$. If the force with which the *north* pole of another magnet repels the ball at the unit of distance be a', then at any other distance r' the repulsion will be $\frac{a'}{r'^2}$. If the two poles act *simultaneously* upon the ball from the same side of it, the ball will be in equilibrium when attraction and repulsion are equal, or when

$$\frac{a}{r^2} = \frac{a'}{r'^2},$$

from which we derive

$$r : r' = \sqrt{a} : \sqrt{a'};$$

that is to say, the distances between the point of equilibrium and the respective poles are directly proportional to the square roots of the strengths of the magnet.

28. This is the law when the poles are points, and when the magnetism of the body operated upon is independent. But in our case the poles are *planes*, and the magnetism of the body operated upon is not independent. Let us conceive two flat poles, charged with opposite magnetisms, to be placed parallel, one underneath the other; the upper one, however, being supposed to offer no obstruction to the passage of the force from the one beneath it. Let the force of attraction between the lower pole, acting by itself, and the sphere of soft iron at the unit of distance be A; the attraction of the same pole at any other distance, R, would, according to the law established in the last section, be $\frac{A}{R}$. The attraction of the upper pole for the ball, when acting on it singly at the unit of distance, being A′, its attraction at any other distance R′ will be $\frac{A'}{R'}$. But when both act simultaneously upon the ball, that which causes attraction by the one will cause repulsion by the other; and when these two forces are equal, the ball will be in equilibrium; that is to say, when

$$\frac{A}{R} = \frac{A'}{R'},$$

or when

$$R : R' = A : A'; \quad . \quad . \quad . \quad . \quad . \quad (2)$$

from which we might be disposed by analogy to infer that the distances of the position of equilibrium from the respective poles are directly proportional to the strengths of the magnets.

29. This conclusion, however, would be altogether opposed to the experimental results detailed in § 3. The error lies in the tacit assumption that the forces exerted at the unit of distance are proportional to the strengths of the magnets. With the theoretic poles this assumption was correct, for there the magnetism of the little steel ball was constant, and hence the attractions between it and the respective poles at the unit of distance were the products of a constant with the strengths of these poles, these products being therefore in the ratio of the strengths. But with the sphere of soft iron every change in the magnet is accompanied by a corresponding change in the magnetism of the sphere, which circumstance puts the proportionality existing in the other case entirely out of the question.

30. It has been already proved (16) that if one magnet have twice the strength of another, and if the latter exercise a certain force upon the iron ball at a certain distance,* the former will exercise the same force at four times the said distance. Hence, adopting the disposition of the poles above described, if the ball be placed at a leaf thickness distant from the weaker pole, to be in equilibrium it must be four leaves distant from the stronger, which is here supposed to be underneath the former; and in general, if m and m' be the strength of any two magnets, and d and d' the distances at which they exert equal attractions upon the sphere, we have

$$\frac{m}{\sqrt{d}} = \frac{m'}{\sqrt{d'}},$$

or
$$d : d' = m^2 : m'^2;$$

from which we infer that *the distances of the point of equilibrium of the sphere from the respective poles are directly proportional to the squares of the strengths of the magnets.*

* It is scarcely necessary to remark that the term distance, as here used, refers to the interval between the magnet and the nearest point of the sphere.

§ 8.

Proposition II.—*To determine the relation between the strength of an electro-magnet and the mutual attraction of the magnet and a mass of soft iron, when both are separated by a fixed distance.*

31. If we suppose the space above the magnet to be intersected by a number of infinitely thin horizontal planes, placed, say the thickness of one of our leaves asunder, each of these planes will denote a certain section of force. Let the force on the first plane be called f; we know that, by doubling the strength of the magnet, the force f will be exerted on the fourth plane; by trebling it, on the ninth plane, and so on. Supposing the ball placed on the ninth plane with the magnetic force thus trebled; let us ask what will take place if it be removed from the ninth plane to the first plane, *without altering the trebled power of the magnet.* The law established in section 6 replies, that at one plane distant it will be attracted nine times as strongly as at nine planes distant. But at nine planes distant it is attracted with the same force by the treble power as at one plane distant by the single power. Hence *upon the same plane* three times the magnetic power will cause nine times the attraction; or, expressed generally, *the mutual attraction of the magnet and the sphere of soft iron, when both are separated by a fixed distance, is directly proportional to the square of the strength of the magnet.*

We have thus arrived, by direct deduction from the foregoing principles, at the well-known law of Lenz and Jacobi, which, expressed in their own words, runs as follows:—

The attraction between two electro-magnets, or between an electro-magnet and a mass of soft iron, is proportional to the square of the strength of the magnetising current.

We shall now bring the deduction to the test of experiment. At the moment the ball gives way, the magnetic power, or, what is the same, the strength of the magnetising current, is expressed by $\tan\beta$. The weight upon the scale-pan is the measure of the force with which the ball is attracted at the same moment. According to the above law, therefore, the quotient $\frac{\tan^2\beta}{W}$ must be a constant quantity, and hence also

$$\frac{\tan\beta}{\sqrt{W}} = \text{const.}$$

For the sake of convenience in the calculation, the quotient is introduced in the latter form in the following table. The angles, as in the former cases, are each the mean of four observations :—

TABLE VIII.

Distance between balls and magnet $\frac{1}{250}$th of an inch.

Ball No. 1.			Ball No. 2.		
W.	β.	$\frac{r \tan \beta}{\sqrt{W}}$.	W.	β.	$\frac{r \tan \beta}{\sqrt{W}}$.
grms.	° ′		grms.	° ′	
50	8 0	2·0	10	7 50	4·37
75	9 55	2·0	20	11 10	4·4
100	11 30	2·0	30	13 30	4·4
150	13 35	2·0	40	15 40	4·4
200	16 0	2·0	50	17 25	4·4
250	17 50	2·0	60	19 0	4·4
300	19 15	2·0	70	20 25	4·4
350	20 40	2·0	80	21 38	4·4
400	21 45	2·0	90	22 48	4·4
450	22 58	2·0			

We here see that the experimental substantiation of the deduction is as complete as could be desired.

32. One important omission, which has been the source of considerable error, occurs in the first utterance of the law by its discoverers.* It is essential to its validity that a '*fixed distance*' shall separate the magnet from the attracted mass. Were the distance 0, we should have the case embraced by our first proposition, which, however, as we have seen, leads to a totally different law. The error, as observed at the commencement of this paper, lies in the assumption that the law which was true for a fixed distance is also true for contact. Lenz and Jacobi observe a certain caution in expressing themselves on this subject, which would lead one to suppose that they also doubted the applicability of the law to the case of contact. In the course of their memoir the following significant remark occurs :—' For the present, at least, we will grant the limitation, *that the magnet and submagnet are not in immediate contact*, but stand about a line apart.' Physicists generally, however, appear to have assumed that the law was meant to be of universal application. This is a point on which Lenz and Jacobi have expressed themselves very explicitly, as the following extract from their memoir testifies.† 'All our precautions,

* Poggendorff's *Annalen*, vol. xlvii. p. 403. † Ibid. p. 411.

however, served only to prove, that when currents of equal power were applied, the results were tolerably coincident. But to the *expected result* they did not lead us; and we could only arrive at the conclusion, that, by strong magnetising, the lifting power—we do not say *the law of attraction*—of two electro-magnets, or of a horse-shoe magnet and its submagnet, is a phenomenon far too complicated to be referred to any such simple law as the square of the strength of the current, or the strength itself.' This very difficulty of arriving at anything like a safe result in the case of contact seems to have suggested the expedient of placing a distance of $\frac{1}{10}$th of an inch between the submagnet and the magnet.

33. If m be the strength of a magnetic pole referred to any unit, and m' the strength of a second pole referred to the same unit, the mutual action of the two poles at the unit of distance will be expressed by the product mm', the said product being negative or positive according as the two poles are of the same or of opposite names. This may be called the fundamental law of magnetism. We have already assumed the magnetism of the soft iron sphere to be proportional to that of the magnet. By aid of this assumption, the law of Lenz and Jacobi can be immediately deduced. Let m_1 be the magnetism of the magnet, and m_0 the corresponding magnetism of the sphere at a given distance. The attraction will be

$$m_1 m_0.$$

Supposing the power of the magnet to be increased n times, the sphere will receive a proportionate increase, and the attraction will then be

$$nm_1 \times nm_0,$$

or

$$n^2 m_1 m_0.$$

In like manner, for any other multiple, n', the attraction will be

$$n'^2 m_1 m_0;$$

hence the attraction in one case is to the attraction in the other as

$$n^2 m_1 m_0 : n'^2 m_1 m_0,$$

or as

$$n^2 : n'^2,$$

which expresses the same as the law of Lenz and Jacobi.

34. In (29) it was asserted that the attraction of a soft iron ball at the unit of distance was not proportional to the strength of the magnet. We now learn that it is proportional *to the square of the strength.* Calling A and A′ the attractions exerted by any two magnets of the strengths M and M′ at the unit of distance, we have

$$M^2 : M'^2 = A : A'.$$

Substituting M^2 and M'^2 for A and A′ in equation (2), we shall have for the position of equilibrium

$$R : R' = M^2 : M'^2,$$

which is exactly the same result as that established in (30) by direct experiment.

§ 9.

35. Table VIII. shows us that a distance of $\frac{1}{250}$th of an inch between the ball and pole entirely changes the law of attraction. In contact, a double current will support a double weight; but at $\frac{1}{250}$th of an inch distance, a double current will support four times the weight. Indeed, it is not until the ball is within $\frac{1}{1000}$th of an inch of the pole that any remarkable deviation from the latter law occurs. From this to absolute contact the passage from one law to the other is gradual.

36. To demonstrate this gradual change, it was necessary to apply a film much thinner than the paper formerly used, and to procure this was a matter of some difficulty. Gold leaf was on many accounts unsuitable. The following expedient occurred to me on observing the extreme tenuity of a gun-cotton balloon. It is well known that a solution of gun-cotton in sulphuric ether, when spread thin and suffered to dry, forms a tough unyielding film. A layer of such a solution was laid, like a wash of water colour, with a camel's-hair pencil upon the smooth flat pole. The layer must have been exceedingly thin, for when dry it exhibited the colours of thin plates with great brilliancy. The ball was brought down upon this, weights from 300 to 900 grammes were laid upon the scale-pan, and the corresponding magnetic powers determined. This done, another wash of the solution was laid on, and the same process repeated. The result may be thus stated:—Through the interval between the

surface of the pole and the sixth layer no law is recognisable. *At the lower limit of this space the attraction of the ball is directly proportional to the magnetising current; at the upper limit, and beyond it, the attraction is proportional to the square of the magnetising current.*

37. I will for the present limit myself to an observation or two on this singular result, and defer the fuller discussion of the subject to a future paper. In the case of contact the fundamental law of magnetism is, to all appearance, contradicted—that law which affirms that the attraction is expressed by the product of the magnetism of the magnet into that of the sphere. But this law supposes that *opposite magnetic fluids* act upon each other. In speaking of the attraction of a mass of soft iron by a magnet, it is usual to say that a north pole excites a south pole, and a south pole a north pole, attraction being the consequence. A cylinder of soft iron 1 inch thick and 6 inches long was laid upon the end of the excited magnet. According to the notion generally entertained, two opposite poles embraced each other at the place of contact. This being the case, we might infer, that on raising a magnetic needle from the centre of the magnet upwards, *that* end of the needle which was attracted below the place of contact ought, upon passing the latter, to be immediately repelled; but there is no such action exhibited. From the centre of the magnet to the top of the soft iron cylinder the same end of the needle was attracted, the combination of magnet and cylinder behaving in all respects as if they were *one continuous mass.** To obtain the opposite polarity implied by the fundamental law, the magnet and the mass of soft iron must be a certain distance asunder. The case of contact, in point of fact, reduces itself to the attraction which the magnet exerts *upon itself*, and this leads us to the threshold of a subject which will be better discussed in a future memoir.

38. I will now turn for a short time to the consideration of the divergencies which exhibited themselves on changing our mode of experiment (18). It has been satisfactorily shown (21) that they cannot be accounted for on the supposition of

* Since making the above experiment, I have learned that the same has been observed by Prof. Poggendorff and M. Van Rees. See Poggendorff's *Annalen*, vol. lxxiv. pp. 213–230.

'lagging magnetism.' Let us imagine the ball placed at the sixth layer of the gun-cotton solution. From this downwards, as it approaches the pole, we have a nearer and nearer approximation to the law of simple proportionality. When a single layer separates the ball from the pole, the law is nearly fulfilled; when this is removed, the approximation is still nearer; but *is the ball then in contact*? Will not the unavoidable roughness which remains after rubbing with fine sand-paper and polishing with emery still make some difference? If we lay a plate of glass upon a convex lens of large radius, so as to produce Newton's rings, a considerable pressure is needed to render the central spot permanently black, or, in other words, to establish perfect contact.* How much more will such a pressure be required in our case, where the surfaces operated with are comraratively so defective! Besides this, the manipulation of the surface must alter in some measure the constitution of the thin outside layer, and render it different from that of the mass of the metal. This remark applies to both magnet and ball. An improvement of the contact would, according to our reasoning, be accompanied by improved results. The contact might be made more perfect by mechanical pressure—with the hand, for instance; but on removing the hand a shaking of the ball is unavoidable, which entirely nullifies the previous pressure. Besides, this pressure may not, and in all probability will not, be exerted in the precise direction of the magnetic force; and in this case, when the hand is raised, the ball will right itself and thus defeat us. The best means of improving the contact seems to be furnished by the magnet itself. If the ball be squeezed tight, in the first instance, by strong magnetic power, the pressure thus exerted is sure to be in the right direction. This, in fact, is what has been done in our first experiments; and these, as we have seen, exhibit a most striking coincidence with the laws affirmed in each case.

39. The imperfections of the surface appear, therefore, to throw the true pole, or attracting plane, some distance beneath that on which the ball rests; which circumstance, when uncompensated by pressure, exhibits itself in the results. This being the case, a small constant would have to be added to the distances denoted by the leaves in those cases where the second

* See Phil. Mag. for December 1850, p. 452.

mode of experiment was adopted; that is to say, in Tables VI. and VII. The alteration thus effected will exhibit itself most sensibly in the immediate neighbourhood of the poles, where alone the discrepancies occur; indeed, its immediate effect will be to lessen these discrepancies; in Table VI. it will make the quotients at the top of the columns less, and in Table VII. it will make the products at the top of the column greater; while at a distance from the poles the change caused by the addition will be scarcely appreciable. There is another slight correction to be made with regard to the paper. When a good many leaves, say fifteen or twenty, are upon the magnet, the space occupied by these is somewhat more than fifteen or twenty times the space occupied by one leaf. On trial with the sphereometer it was found that fourteen leaves occupied nearly fifteen times the space of one leaf. Taking this into account, and supposing the true pole in the case of ball No. 2 to be about $\frac{1}{1000}$th of an inch beneath the surface, and for the large ball a little deeper —for it must be remembered, that the position of the pole depends as well upon the surface of the ball as upon the magnet—the discrepancies arising from the second mode of experiment will entirely disappear.

40. Some of the laws expressed in the foregoing memoir will naturally be limited by the dimensions of the magnetic plane from which the attraction proceeds, and also by the size of the attracted ball. If the views of M. Müller, already adverted to, be correct, a point of saturation for very small balls must soon exhibit itself,* after which the law would be no longer applicable. As far as I am able to judge, the balls used in the present case, although of very different volumes, follow the same law.

In stating the case, I have made use of those experiments merely which appeared best calculated to illustrate the several laws and the apparent deviations from them. To the latter, indeed, I have given the most prominent place, as their explanation appeared to me to be most important. The experiments recorded constitute, however, but a small fraction of the number actually made. My aim has been to embrace in one investigation the whole of a subject whose separate details

* Some interesting remarks on this subject occur in a paper by Professor W. Thomson, Phil. Mag. vol. xxxvii. p. 241.

have occupied the attention of many experimenters. One law alone of those expressed in the foregoing pages has been heretofore established—that of Lenz and Jacobi; and this, as we have seen, forms a link in a chain of laws, or rather a deduction, which flows *à priori* from the combination of the 3rd and 4th propositions. The laws of magnetic action, at distances in comparison with which the thickness of the magnet vanishes, have been long known. But the complementary portion of the subject, which embraces the laws of action at short distances where the thickness of the magnet comes fully into play, has, so far as I am aware, hitherto eluded the grasp of experiment and formed a subject of mere puzzling conjecture. The want here experienced it has been the object of the present inquiry to supply.

The principal results may be summed up as follows:—

I. The mutual attraction of a magnet and a sphere of soft iron, when both are in contact, is directly proportional to the strength of the magnet.

II. The mutual attraction of a magnet and a sphere of soft iron, when both are separated by a small fixed distance, is directly proportional to the square of the strength of the magnet.

III. The mutual attraction of a magnet of constant strength and a sphere of soft iron is inversely proportional to the distance between the magnet and the sphere.

IV. When the distance between the magnet and the sphere varies, and a constant force opposed to the pull of the magnet is applied to the latter; to hold this force in equilibrium, the strength of the magnet must vary as the square root of the distance.

I have, in conclusion, to express my deep sense of the kindness of Professor Knoblauch, who, during this investigation, permitted me to occupy three of his rooms, and placed his extensive and beautiful collection of apparatus entirely at my disposal.

Marburg:
January, 1851.

These statements must be understood to be true only within the limits of current strength and distance here employed. Even

thus limited they are still very curious, and suggest thoughts which need further working out. Many years ago I drew attention to an objection urged by M. Dub against the combined propositions that the attraction in the case of contact is proportional simply to the strength, and, at a fixed distance, to the square of the strength, of the magnet. He justly pointed out, that this, if universally true, would end in making the attraction at a distance greater than the attraction in the case of contact. Poggendorff has shown how the reaction of the attracted iron upon the electro-magnet modifies the law of attraction. The manner in which these various propositions intertwine with and sustain each other strikingly illustrates the support which principles not universally true may lend to one another. I think I can recommend the experimental methods employed in this inquiry as useful to a student who wishes to make himself practically acquainted with the subject of electro-magnetic attractions.—J. T., 1870.

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